A

TREATISE

UPON

ARTIFICIAL ELECTRICITY,

IN WHICH ARE GIVEN

SOLUTIONS of a Number of interefting ELECTRIC PHOENOMENA, hitherto unexplained.

TO WHICH IS ADDED,

AN ESSAY

ON THE

MILD and SLOW ELECTRICITY which prevails in the Atmofphere during Serene Weather.

Tranflated from the ORIGINAL ITALIAN of

Father GIAMBATISTA BECCARIA,

Profeffor of Natural Philofophy in the Univerfity of TURIN.

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MDCCLXXVI.

I think I cannot do better than to premiſe to this book, the ſentiments contained in the letter with which I accompanied the ſame, when I ſent a copy of it to Dr. Franklin.

Turin, 20*th*.
of *May*, 1771.

I THANK you, moſt excellent Sir, for the exact deſcription of your new and really harmonious muſical inſtrument with glaſſes, which you have ſent me (to you it is given to inform mankind of the true principles of the Electric Science, to encourage them againſt the terrors of thunder, and charm their ears with a moſt ſweet muſic); and I will moreover venture to thank you likewiſe, in the name of our Italy, for having in conſideration, as you are pleaſed to ſay, of our harmonius language, given the appellation of *armonica*, to your agreeable inſtrument*. With regard to me, I do not know how to teſtify better my gratitude to you, for the many marks of your kindneſs, than by ſending to you this new product of my labour, and accompanying it with a wiſh, that it may anſwer your firſt opinion of it, as you were pleaſed to expreſs it.

As for the bigneſs of this volume, I hope it will not tire you; beſides the common excuſe, that I have not had time enough to write with more brevity, I have had rather to repeat, than to quote: and to ſay the truth, I do not think that the number of both experiments and ſolutions contained in this Book, bear a very unequal proportion to the ſize of the volume.

I hope you will not be ſurpriſed that I conclude this book with expreſſing wiſhes that farther new confirmations of the Theory contained in it, may be procured; you too

* See Dr. Franklin's Works, page 438, fifth Edition.

well teach, by your own example and authority, how ſlowly and difficulty our ſenſes allow us to proceed in the attainment of natural knowledge, which is inexhauſtible. And how many new informations does not Science, however comprehenſive, want to this day? We know how to meaſure the velocity of light, but we are ignorant how it proceeds. We can define the order of our mundane ſyſtem, and yet we are ignorant of that force which both parts and unites it, &c.

But what ſhould be the conſequence, if new informations with regard to Electricity happened to produce another different theory? The whole ſeries of my experiments to this day, takes any ſuch ſuſpicion from me; in ſuch caſe however, the value of this book, if indeed it may be ſaid to have any, would ſubſiſt. The experiments would remain, together with their collection and unity.

I hear that you are preparing to go back to your native country. Whether America or Europe poſſeſs you, preſerve yourſelf to the world, to ſcience, and the lovers of it. Wherever you may be, I always ſhall be the moſt obſequious admirer of your eminent merit.

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OF

ARTIFICIAL ELECTRICITY.

SECTION I.

Of the Theory of Artificial Electricity, eſpecially in *deferent* or conducting bodies, deduced from the circulation of the electrical fire in the ordinary apparatus.

CHAP. I.

Compendium of the theory.

1. FIRST, the two maſters of true learning, Obſervation and Experience, have ſhewn this century, that within the ſubſtance, and on the ſurface of all bodies, a particular fluid element is diffuſed, which is of a diſtinct nature, and in the atmoſphere governs the clouds, lightning, thunder, &c.

2. This element is diffuſed in bodies in proportion to their natural capacity, it remains there balanced with itſelf, has no motion, and produces no impreſſion on our organs.

3. But when the power of nature, or of art, changes the natural proportion in bodies, by tranſporting part of that fluid from the one to the other, then finding itſelf unbalanced, it vehemently endeavours to ſpread and diffuſe itſelf from the bodies in which its proportion is become greater, to thoſe where it is become leſs.

4. And then, either that fluid is prevented, by reſiſting bodies that ſurround it, from diffuſing itſelf to an equality; or, actually ſurmounting thoſe reſiſtances, becomes able to diffuſe itſelf; and in both theſe caſes it manifeſts, either its ſtate of inæquilibrium,

or its actual diffufion, with certain peculiar figns, which are commonly called *figns of electricity.*

5. That element, which is now acknowledged as the principle of the greateft effects in nature, many of which art, borrowing help from it, has enabled itfelf to imitate, is the very fame of which the ancients had indeed fome knowledge, but were acquainted with only a few of its effects and motions, in a few particular bodies, chiefly in amber, or *electrum*; whence it has received, in thefe days, the name of electrical fire or fluid; the whole of its effects is called *electricity*; and the knowledge of thefe may be confidered under two heads, and called Artificial Electricity, when and where it is excited by art; and Natural Electricity, when it is excited by nature herfelf.

6. Therefore, if the furrounding refifting bodies hinder the electrical fire, when unbalanced, from fpreading itfelf to an equality, then it manifefts this its unequal ftate, by making the moveable parts of the electrified body, diverge from each other, and tend towards bodies that are not electrified. I call that fign, an electrical fign of fimple preffion, or of mere inequality, or of *preffing electricity.*

7. Hence arofe the idea of annexing to the bodies that are electrified, two threads, in order to eftimate their prefent electricity from the degree of their divergency. I ufe for that purpofe Flanders thread, extremely fine and flexible, two or three inches long; and, to prevent their entangling together, I fufpend to each of them a little ball made of the pith of elder. This I call an *electrofcope*: fuch an electrofcope does not exceed in weight the fourth part of a grain.

8. But when the electric fire is actually able to furmount the furrounding refiftances, and from the body in which it finds itfelf in a greater proportion, actually diffufes itfelf into bodies wherein its proportion is lefs; then it manifefts itfelf with new figns, with particular motions, fparks, a wind, &c. all which are called vivid figns, figns of a prefent diffufion, figns of a *vivid electricity.*

9. And with regard to its motion; if around the electrified body, or near it, there are fome moveable bodies, which the unbalanced electric fire may direct, extend, fufpend, difpofe, or throw into its way

way from the electrified body to bodies not electrified, that fire, by its peculiar efficiency, actually directs, extends, suspends, or throws those moveable bodies; and, in short, disposes them throughout the resisting medium in the properest manner, in order to diffuse itself through them, with the greatest velocity its nature, and the *traductive* quality of those bodies, can allow.

10. But if around, or near, the electrified body, there are no moveable bodies, apt to transmit the electric fire, and which this may dispose in its way; and if at the same time this unbalanced fire is able to throw aside the resisting medium that parts it from bodies not electrified; then that fire actually drives this medium in that part where it resists less, with a noise and sparkling proportioned to its own density and quantity.

11. This sign arising from *sparks*, is often complicated with that resulting from *motion*; that is, whenever the series of bodies which the electric fire disposes in its way, is not continued enough, or of a sufficient size, invisibly to transmit the spark through its own substance; then, in the places where such a series is either too scanty, or interrupted, the fire rushes out, and manifests itself with a proportioned noise and light.

12. And it is in such a passage, through bodies of a small size, that the electrical fire may instantly produce in such bodies all the identical effects which the common fire can, though in a tract of time, produce in them, respectively; that is, the electrical fire condensated in a spark, alters these bodies proportionably to the density with which it presses through them, and conformably to their nature, in the same manner as the common fire would do, but with such a superior quickness, as it may be said to perform it in the shortest *tempusculum* of its passage.

13. Lastly, the third electric sign of an actual *diffusion*, is a real wind that blows, especially from points annexed to electrified bodies, or presented to them. The reason is this: those points throw into the contiguous air, or draw from the same, the electrical fire with such a force, that in both cases they repel from themselves the air, either loaded with part of the fire of which themselves are too replete, or deprived of its own fire, if these bodies were themselves in a *wanting* state; and these points, while

they thus drive away the air next to them, throw into, or draw from, the new air which laterally ſucceeds, the electrical fire, and effect it in ſuch a divided manner, that the fire, whether drawn in, or thrown out, produces but a little noiſe, and affords but weak lights; but all this is effected with ſuch rapidity, that a great quantity of fire is really thrown out, or drawn, in a very ſhort tract of time.

14. I ſay, weak lights, and ſuch they are if we conſider the much leſs vivacity of the ſparks. Beſides, the light which appears above a *wanting* point, that draws in the fire, is of a ſmall bulk, ſo that I expreſs it by the name of *little ſtar*; but the light emitted by a *ſuperabounding* point from which the fire ſparkles out, is much larger, conſequently I call it, the *bruſh*. And I have made uſe of ſuch a difference between the electric lights, in order to divide the *ſuperabounding* bodies which throw out the electric fire, and which I call electrified *per exceſſum*, from the *wanting* bodies which draw it in, and which I call electrified *per defectum*. But of this I ſhall treat more at large when I give the demonſtration of the theory, of which this is only a compendium.

15. In order to continue this compendium, it is now neceſſary to explain what are the *reſiſtances* which I have mentioned, that prevent the electrical fire from diffuſing itſelf to an equality, and ſtop and keep it in a ſtate of *inæquilibrium*.

16. Now, experience has ſhewn, that the electrical fire, though univerſally diffuſed in all bodies, has not the ſame mobility in all. In metallic bodies, the electrical fire moves itſelf with the greateſt facility and freedom; ſo that if the ſmalleſt quantity of it is added to a part of a metallic body, it immediately diffuſes itſelf to an equality throughout the whole ſubſtance of the ſaid body, and there reſults an increaſe in the denſity of the electrical fire, proportioned to the quantity that has been added, directly, and the capacity of the body, inverſely. Likewiſe, if any quantity of fire be taken from a part of a metallic body, the remaining fire directly diffuſes itſelf to all parts of the ſubſtance; and there reſults a diminution in the denſity of the fire, proportioned to the ſubtracted part, directly, and the capacity of the body, inverſely. It is chiefly in conſequence of ſuch a propriety that metallic bodies are called *con-*

ductors,

ductors or *deferent*; and indeed they are fo to fuch a degree only as long as they actually keep their metallic form.

17. Next to metallic bodies, fuch liquids as are lean and not inflammable, are conductors, though in a much lefs perfect degree: and confequently animals, plants, which all abound with the faid liquids, and other bodies that attract and retain the fame, are capable of conducting likewife; and, indeed, the lefs thefe bodies have of moifture in themfelves, the lefs they poffefs the power of conducting. Electrofcopes may ferve as an example, which in very dry weather move but very flowly, becaufe the electrical fire can then have but a retarded motion through their dried fibres.

18. Hence, a mixture, whether natural or artificial, of humid particles, and alfo a mixture of metallic particles, renders bodies capable of conducting in any proportional degree. Walls, the ground, bricks, ftones of a porous nature, are deferent only in confequence of either of thofe two caufes, or of both. We fhall fee how lightning choofes out in its way fome particular ftones, only in confequence of their being of a particularly metallic nature.

19. However, it muft be confeffed, that there are fome forts of bodies which are in fome degree *deferent*, and which, neverthelefs, are not indebted for that their propriety to any moift or metallic particles they have within themfelves: fuch undoubtedly are charcoal, and any glafs recently taken from the furnace, as Dr. Prieftley has demonftrated in his valuable Hiftory of Electricity, printed in London, &c. 1767.

20. In fhort, it appears, that in this cafe likewife, nature proceeds by a gradual feries: and indeed there are bodies which from the greateft degree of deferency or *conductivenefs*, gradually defcend to the fmalleft; and the two extremities, of bodies perfectly *deferent*, and of bodies perfectly *infulating*, may be faid to join.

21. Among *infulating* bodies, air, to the mortification of reafoners, and the no fmall trouble of experiment-makers, muft be ranked firft; which, however rare, actually ftops the electrical fire, that runs with fo much eafe and celerity through the moft compact metals. Air, therefore, is in its nature infulating; and it being

every

every where plentifully diffufed, fupplies, in experiments, the greateft part of the *infulation*, fince it is fufficient to fufpend in the air with infulating ftrings, or raife with infulating fupports, the conducting body, to have it completely infulated.

22. Therefore, fince the electrical fire can run wherever it finds any moifture, air (and the fame muft be underftood of all other *repreffing* bodies) will be able to infulate, but only in proportion as it is dry. This is an obfervation of which I cannot remind too much thofe who might pretend to fucceed in all feafons, and even in all places, in experiments of a delicate nature. I beg leave to introduce here an example: in the bottom of the electric *well* *, an infulated body contracts no electricity; but if I only breathe in the *well*, the fame body will contract a degree of electricity.

23. Experiments, indeed, may be made in fpite of the weather, in an air artificially warmed and dried; but if fuch an air remains entirely clofe, the tranfpiration of the perfons in the room, independently of the vapours from the fire, will be enough to render it in fome degree damp: if it circulates, then the heat will indeed leffen, but not annihilate the damp vapours. Confequently, fuch experiments only may be attempted as do not require an exact infulation, nor the utmoft intenfity of electricity.

24. Now to refume our enumeration; among bodies naturally infulating, I principally reckon the various fpecies of fulphur, and generally all refins that are not foluble in water, and do not attract it, as other bodies do.

25. But for this fame reafon, and for their fuperior folidity, as well as figurability, I give the preference to woods that, with the help of art, are rendered capable of infulating, by being dried at feveral times in an oven, and at every time anointed with oil of linfeed, fo that there refults from it a varnifh clofe enough to preclude any entrance to exterior moifture. Thefe I call *oiled woods*.

26. Befides, there are, if they might be kept free from dampnefs, feveral other bodies very ufeful for infulating. In order to infulate *by fufpending* them, fmall cords of wool are very convenient, and much more thofe of the beft fort of filk, which are not impreg-

* The experiment of the electric *well* is defcribed in the paragraph, 442.

nated

nated with ſalts that may attract dampneſs, as for inſtance are thoſe dyed black.

27. In order to inſulate bodies by *raiſing* them, the various ſorts of cryſtal, and glaſs of all kinds, are made uſe of. I ſhall take this occaſion to obſerve, that metals very poſſibly owe to their phlogiſtic their propriety of conducting, ſince by vitrifying them, that is, by taking from them their phlogiſtic, they become capable of inſulating; and by bringing them back to their metallic form, that is, by rendering them their phlogiſtic, they are again made capable of conducting. But with regard to ſpirits, for inſtance the ſpirit of wine, it appears, that their phlogiſtic ſerves, on the contrary, to render them capable of inſulating.

28. Precious ſtones can alſo inſulate; but are not made uſe of on account of their ſmallneſs. The excremental parts of animals, hair, feathers, nails, horns, when freed from moiſture, can likewiſe inſulate, as well as the fibroſe parts of animals and plants, which, as we obſerved before of wood, become capable of inſulating whenever they are rendered ſufficiently dry; and that they may conſtantly keep ſo, it will ſuffice to varniſh them in any manner you pleaſe, as we have ſaid of wood.

29. But this diviſion between *inſulating* and *conducting* bodies, will bring on another, which ſhews what ſort of bodies are capable of *unbalancing* the electrical fire, and diffuſing, if I am allowed the expreſſion, its *inæquilibrium*; and, beſides, in what manner they can effect this, which indeed is the principle of all the electrical phenomena. In ſhort, the rubbing of inſulating bodies, or bodies *electric per ſe*, againſt conducting or *non-electric* bodies, is, till now, almoſt the ſole means that we know of tranſporting the electrical fire from one body into another, and putting it out of its natural æquilibrium.

30. I ſay, the almoſt ſole means; becauſe the precious ſtone known by the name of tourmalin, is electrified without any other operation but the heating it, and letting it cool. That ſort of ſtone is formed of ſmall long-ſhaped cryſtals, which, being united length-ways, make it terminate in a round ſhape, with rectangular ſides, diſpoſed in angles, ſome pointing outwards, others inwards. Thoſe of this ſort of gems which come from the Eaſt-Indies, and eſpecially

cially from the iſle of Ceylon, are of a brown dark red colour, which inclines to the colour of coffee: thoſe which come from South America, are of the ſame colour with the ruby, or paler; ſometimes of an orange colour, or even greeniſh.

31. Thoſe gems are the only kinds of body that may be electrified, as far as experience informs us, by the ſole increaſe or decreaſe of their heat. Therefore, though philoſophers have generally aſſerted that ſulphurs, reſins, and mixtures made of them, may be electrified by heating, or melting them, and, if carefully kept, will retain for years their electricity, yet I think I have found the untruth of ſuch an opinion, and the reaſon that has cauſed it to be univerſally received. This is, that the ſmalleſt poſſible friction is ſufficient to excite a degree of electricity, in ſuch bodies as have been carefully dried: in moving the vaſe in which they were melted, in taking them out of it, in unfolding the paper in which they were kept, or even in placing them upon a table, it is next to impoſſible to avoid any ſmall friction, ſufficient to lead into a miſtake. But if you take one end of a ſtick of ſealing-wax, of ſulphur, &c. and heat it in any degree, and then let it cool, holding it all the while by the ſame end, and taking care not to handle it otherwiſe, or let it touch any other body, you will find yourſelf abſolutely unable to draw with it the ſmalleſt hair, or thread; but whenever the ſtick has been in contact with any other body, it will immediately attract them.

32. It is true, that in cutting with a knife lumps of ſulphur, or ſealing-wax, in order to give them a round ſhape, I find that they become electrified; but I do not take this to be in the leaſt a new way of exciting the electrical fire: a knife does not ſeparate the prominent parts of a body, but by ſcraping and rubbing the inferior ones; therefore, in this caſe likewiſe, electricity is excited by friction.

33. Proceeding farther, I will ſay, that electricity is excited by the friction of an inſulating, or *per ſe electric* body; eſpecially when this friction is made with a *deferent* or conducting body. With regard to theſe we may obſerve, that two inſulating bodies may very well, when rubbed againſt each other, excite electricity; but in order to render it continued, the inſulating body with which the

the friction is made, muſt have very near it a conducting body, inceſſantly to ſupply it with what fire the rubbed inſulating body takes from it; or inceſſantly to receive and diffuſe away the fire which the ſame communicates to it.

34. As to the friction of two conducting bodies with one another, it is an undoubted fact, that it cannot produce the leaſt electricity. I have often, being placed upon an inſulated ſtool, rubbed, ſcraped, or ſawed balls or cubes of various metals, and there never reſulted the leaſt ſigns of electricity, as would undoubtedly have been the caſe, if electrical ſigns proceeded from either an abſolute augmentation, or an abſolute diminution of the electrical fire, and not from a ſimple tranſlation of the ſame, from one body to another. According to ſuch an hypotheſis, there would have reſulted either an exceſs, or deficiency in the ball, or in the ſtool, or in my body; but the truth was, that the fire that was tranſported by any friction whatſoever, from the one to the other, inſtantly flowed back to its former place.

35. After all, it appears that inſulating bodies, at leaſt moſt of them, require to be rubbed with conducting bodies. I have immerſed in ſome quickſilver contained in a deep, narrow, ſemicircular box of oiled wood, the plate of glaſs, which I now uſe in my apparatus inſtead of a globe, or cylinder; and having this plate turned round as I uſe to do, in that highly conducting fluid (which communicated with the floor through an iron wire) it afforded me a very ſtrong electricity; to which I muſt add, that I could not move it but ſlowly, for fear of making the mercury leap out of the box.

36. This is an inconveniency to which the excellent rubbing amalgam, adviſed by Mr. Canton, in the 52d volume of the Philoſophical Tranſlations, is not liable. Upon a ſilk cloth, covered with wax, a little amalgam muſt be ſpread, made of mercury and tin, powdered with chalk; upon this the friction is to be made: that cloth, thus prepared, is extremely uſeful for rubbing a tube of glaſs, or an oiled ſtick; when it is applied to cuſhions, it renders them extremely proper for rubbing the globes and plates, uſed in the ordinary complete apparatus, which I am now going to deſcribe.

37. But I ſhall firſt give a ſummary idea of the apparatus itſelf, and of the theory of their functions, by relating an experiment which is the ſame with which Dr. Franklin originally explained the ſaid theory. There are three perſons A, B, C: the two perſons A, B, ſtand inſulated upon two ſtools, armed with ſupports of glaſs (Tab. I. fig. 4.) the perſon C ſtands upon the floor, in the middle of the two inſulated perſons. The perſon A rubs a tube of glaſs and preſents it to the perſon B, who reciprocally preſents to him a point. This is the whole of the operation through which the tube, and the two perſons become electrified: let us now ſee which way this is effected. 1. The rubbed tube takes the fire from the rubbing perſon A. 2. The rubbed tube, preſented to the perſon B, imparts to him the fire it has taken from the perſon A: and thus, there reſults an electricity *per deficiency*, or negative electricity in A; and an electricity *per exceſs*, or poſitive electricity, in the perſon B. A can receive fire from C, who ſtands on the floor and has the ſame natural proportion of fire with it, or from B, who has a far greater proportion; B can impart ſome of his fire to C, who has but the natural quantity, or to A, who has ſtill leſs.

38. If the perſon A rubs an oiled ſtick, the ſtick will depoſit its fire into the perſon A; and the ſame, being preſented to the point held by the perſon B, will draw from him his natural fire: A will be electrified poſitively; B negatively.

39. But in ſuch an experiment the tube will draw but little fire from the body of the electrified perſon A, when inſulated; and therefore will alſo impart leſs of the ſame to the perſon B. Likewiſe the oiled ſtick will give but little fire to the perſon A; and conſequently extract leſs from the perſon B. But if A comes to communicate with the perſon C, then the tube becomes enabled to draw a much greater quantity of fire from the much larger capacity of the floor; then likewiſe, the oiled ſtick will have room to diffuſe away more of its own fire: hence the tube, or the ſtick, will be able to create in the perſon B a greater, either *exceſs*, or deficiency.

CHAP.

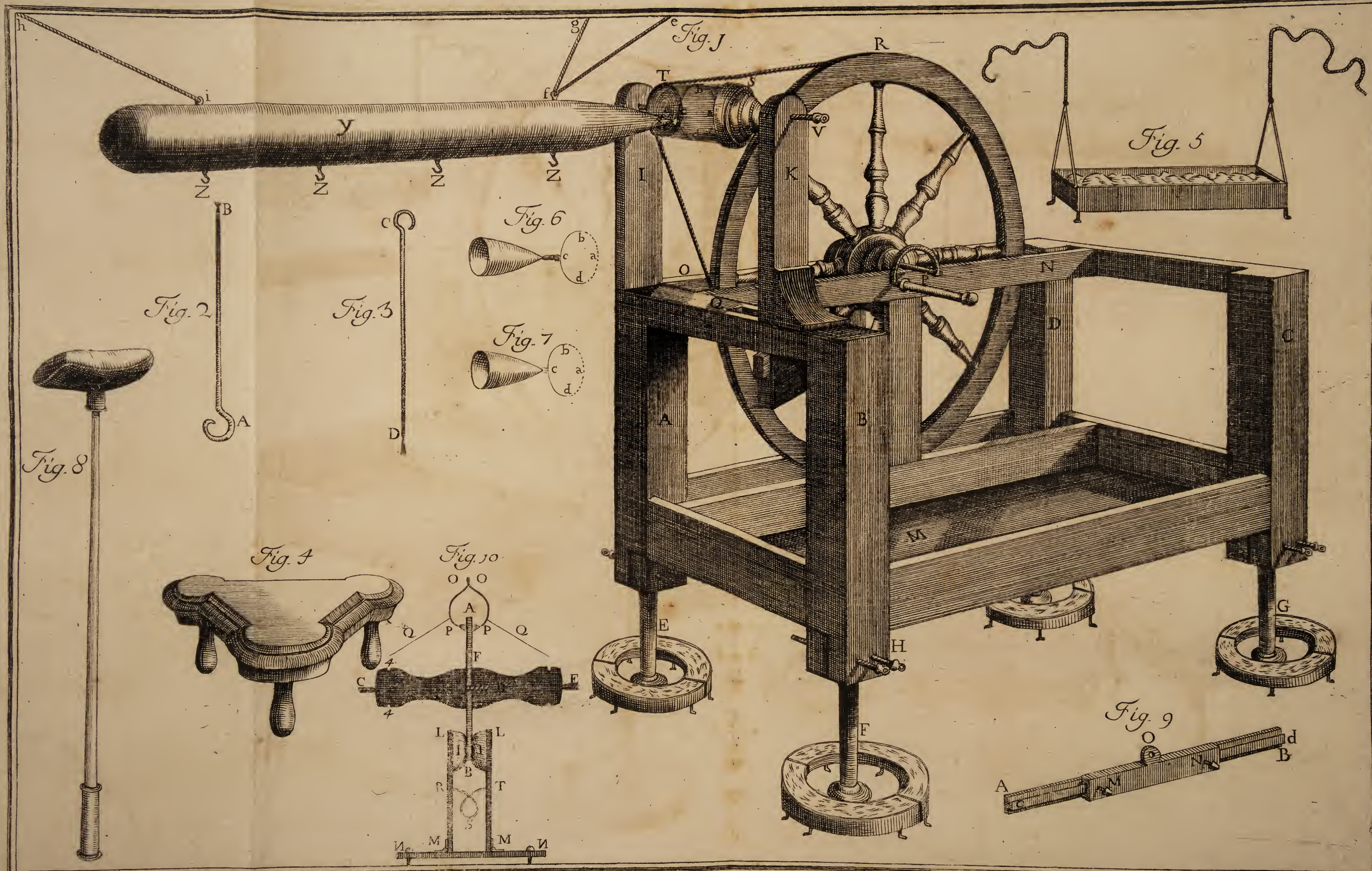
Fig. 1
Fig. 2
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CHAP. II.

On the electrical apparatus.

40. AN infulating, or *per fe electric* body, and two conducting bodies, therefore conftitute an electrical apparatus: viz. an infulating body which is to be rubbed by a conducting body, and have a communication through its rubbed part with another conducting body; and two conducting bodies that may be infulated at pleafure, either one, or both of them. And fuch an apparatus will be the more perfect as its fize and exactnefs taken together, are greater.

41. Here follows the defcription of the apparatus I make ufe of. 1. In T S, between two poles an infulating body is to be fitted, which in the cafe of the figure Pl. I. Fig. 1. is a cylinder of glafs. 2. The frame, made of little beams, and boards, A B C D M N O I K, is deftined to fupport, firft the infulating body T S, then the wheel R, and laftly it muft be able to receive in M, the man who turns the wheel, and him who rubs the cylinder. As thefe bodies, that is the frame of wood, and the man who turns the wheel, all communicate with the man who rubs, and may, all together, be infulated at once, they may be faid to belong to the rubbing *conducting* body; and I comprehend their whole affemblage under the name of the *Machine*. 3. Laftly, the hollow tube of brafs Y, twelve feet long, and a foot in breadth, which has one of its ends fhaped like a hemifphere, and with the other terminating in a conic point, ftands in *c* near the equator of the rubbed cylinder, is the other deferent, or conducting body, in which the rubbed cylinder is to diffufe the electrical fire which it draws by the effect of the friction, (the property of glafs is to draw fire) from the man that rubs, or from the machine, or through it, from the floor. This deferent body, of which the form and fize may be varied at pleafure, is, indeed, ufually called the *prime conductor*; however, whatever change may have been introduced fince in the thing itfelf, I fhall retain the old name of it, and call it the *Chain*; the firft perfons who made electrical experiments having commonly ufed a metallic chain.

42. This

42. This is the whole of the apparatus, let us now deſcribe more exactly its parts: I ſhall begin with the machine. Firſt of all, the materials that compoſe it muſt have a certain ſolidity, that it may be kept from being agitated and ſhaken by any motion, however quick of the wheel.

43. Secondly, the whole body of the machine, and the men who belong to it, muſt be able to be inſulated, that experiments may be made on the negative electricity, which the friction of the glaſs will create in the aggregation of bodies that compoſe the machine when it is thus inſulated. Supports, or little columns of ſolid glaſs, have appeared to me extremely fit for that purpoſe: they have but a ſmall ſurface, which conſequently can draw but little moiſture, and the poliſh of the ſurface makes even ſuch moiſture to be very eaſily wiped off with a warm cloth: ſmall columns of oiled wood would not be ſolid enough, or at leaſt would ſhake. It only remains now ſtrongly to unite the columns to the machine, and to the floor. In order to effect this, it is neceſſary firſt, to make the columns larger towards their head, which is to be inſerted in the under part of the beams; ſecondly, the extremity of the beam muſt be ſawed, firſt vertically, and then laterally, as may be ſeen in H, ſo that a part of the beam may be taken off. Thirdly, in the two ſurfaces of the beam, and of the piece taken from it that have been parted, two grooves muſt be formed, which, when joined, form a round hole, and are to receive the head of the column of glaſs: the latter muſt be garniſhed with leather, and when it has been inſerted in the hole above, two ſtrong ſcrews (thoſe in H are repreſented looſe) drawn from the oppoſite part by their females ſcrews, will faſten the whole with a great ſolidity. In order to prevent the columns moving about on the floor, they muſt be inſerted in a little pedeſtal of wood, which by the help of three points of iron will be fixed in the floor. When I propoſe to make experiments, notwithſtanding the unfavourableneſs of the weather, I uſed to ſurround each column with two little ſemicircular boxes of iron, which, when united around it, form a circle, and in theſe I put ſome hot cinders, in order to keep the column dry, without rendering the air *deferent*.

44. Thirdly,

44. Thirdly, the wheel muſt be ſo diſpoſed as to be able to be brought nearer to the cylinder, or moved farther from the ſame, according as the rope will happen to be contracted in damp, or extended in dry weather. To that end . . . . but all this will be better underſtood by a ſimple inſpection of the plate (Pl. I. Fig. 1.) than by any explanation that might be given here; and any intelligent artificer will, of himſelf, hit upon the propereſt mechanical means to execute the whole with exactneſs.

45. Fourthly, the machine muſt carry the cylinder T S. (Pl. I. Fig. 1.) To that end, the two ſtout ſupporters I, K, are raiſed; they muſt be pretty high, to prevent the electricity of the cylinder T S, and of the chain Y, from flowing again into the machine *.

46. All theſe things above belong to that aſſemblage which we have called *the machine*; as likewiſe do the two armatures of wood, which are to be adapted in T and S, to the head of the cylinder, in order to ſettle it upon the poles, and the one of which is to receive around it the rope of the wheel. Of thoſe armatures I ſhall ſpeak as ſoon as I have explained what are the requiſite qualities of the cylinder. The cylinder muſt be choſen round, thin, about nine or ten inches in diameter, and care muſt be had that it has been annealed. The roundneſs is neceſſary, to the end that when it has been well ſettled upon its pivots it may touch the rubbing ſubſtance with all parts of its ſurface. The thinneſs of the glaſs, which circumſtance muſt not be to the detriment of the neceſſary ſolidity, makes the cylinder to be electrified with more facility and vivacity; the greatneſs of its diameter prevents its being heated ſo ſoon by the friction, becauſe the ſame part does not turn again to the rubbing body (if the velocity of the revolutions be kept conſtant) but after intervals of time that are the longer, as the diameter of the cylinder is larger; the bigneſs of the cylinder having moreover this good effect that the chain may ſtand at a greater diſtance from the rubbing body; and thus the fire which it receives does not flow back into the latter. On the other hand, as the reſiſtance ariſing from the friction increaſes as this is made at a greater diſtance from the centre of motion, and likewiſe, as cylinders of a larger ſize than ordinary, cannot have ſuch an exact round ſhape, it is better to have

* For the propereſt manner of faſtening them to the machine, ſee Plate I. Fig. 1.

them

them of a middling diameter. Laſtly, with regard to cylinders or globes* made of a glaſs which has not been annealed, that is, which never paſſed through an almoſt inſenſible gradation from the heat of the furnace, to that of the open air, I ſhall obſerve that the want of a caution like this, has appeared to me always to be the cauſe of the breaking of globes or cylinders, (particularly if their thickneſs was ſomewhat greater than ordinary) and flying in ſhivers, to the danger of the aſſembly. I always conſidered thoſe accidents as owing to a cauſe analogous to that which produces the rupture of the Bologna flaſks, and of the *Batavic drops:* I think, that the friction actuates the principle which cauſes the breaking of cylinders or globes; which principle is no other than the fire in them, a great quantity of which is excluded from the exterior ſurface, then rapidly contracted and obſtructed. Now, as glaſs which has been annealed is not ſubject to break, I thought that a ſimilar caution would preſerve either globes or cylinders; and, in fact, let the reaſon of it be what it will, none of thoſe I have got to be thus prepared, has happened to break in my hands.

47. Beſides the danger of having the cylinder broken on account of the rawneſs of the glaſs, there is another danger which is no leſs, that of having it leap out of the pivots on which it revolves. To that end, care has been taken that the ſupports I K ſhould be ſtrong, in order to prevent their receding from each other; the ſcrews likewiſe have been made thick, and are immovably faſtened with female ſcrews: this done, the only remaining care is to inſert the point of thoſe ſcrews into ſcutcheons of metal, out of which no agitation whatſoever may cauſe them to eſcape. Such ſcutcheons are to be inſerted in the *armatures* of wood which are fitted to the heads of the cylinder or globe, and are beſides faſtened to the ſaid armatures with three ſmall ſcrews, fixed through three little tails which come out of the body of the ſcutcheon.

48. What muſt be the ſhape of the ſaid armatures, and how they are to be united to the cylinder, is ſcarce worth ſaying. They muſt be made ſo as to embrace a pretty large portion of the head of the

* I prefer cylinders only becauſe they are uſually of a more exact round ſhape, and may at the ſame time be procured long enough to allow the rubbed *zone* to remain at a ſufficient diſtance from the armature of wood fitted to them.

the cylinder, or globe; either of them, or both of them, muſt beſides have one or more deep grooves cut into them, which are to receive the rope of the wheel. When I intend to fit ſuch armatures to the globe or cylinder, I heat them, as well as the heads of the latter; within their cavity I pour ſome melted cement; then I apply them to the warm heads of the cylinder, or globe, which I faſten, thus armed, between their ſcrews, or pivots. A rule placed between the two ſupports I K aſſiſts me in fixing the armatures in ſuch a manner as to make the cylinder revolve with exactneſs, and touch, with all the parts of its rubbed zone, the body that performs the friction.

49. However, in all that I have hitherto ſaid, concerning the choice of either globes or cylinders, and the beſt way of uſing them, my only inducement has been, that I looked upon them, notwithſtanding the inferior quality of their glaſs, as being uſeful to thoſe who cannot procure the apparatus of a plate of cryſtal, which apparatus is now uſed to a much greater advantage than globes or cylinders. I now make uſe of a plate which is about a foot in diameter, as in A B (Pl. I. Fig. 10.) it is poliſhed like a looking glaſs, and very even, which circumſtance may eaſily be diſcovered with a rule, or with a ſtretched thread. The ſaid plate is cut round, and bored in the centre, ſo that the ſcrew D, which comes out from the wooden ſupport C D, may paſs through it, and be inſerted in the female ſcrew bored in the other ſupport F E G. Theſe two ſupports are armed like the cylinder, with their ſcutcheons of metal, and are ſettled in lieu of it upon the ſame pivots, and then the rope of the wheel is ſet around the groove 4, 4, of the wheel R. In O P and O P are two rods of braſs, which from the head of the chain preſent their points to the edges of the plate, and are faſtened with two ſilk ſtrings to the tops of the ſupports I K.

50. I commonly rub the plate by taking it between the index and the thumb of one of my hands, or between the moſt fleſhy parts of both my hands, which, when they happen to be ſomewhat damp, prove excellent cuſhions; a little moiſture being almoſt as efficacious as the amalgam.

51. However, when I propoſe to continue the friction for a long while, in which caſe the plate contracts much heat, then I make uſe

ufe of the cufhions I, I. (Pl. I. Fig. 10.) made of Turky leather, ftuffed with bits of cloth, and well rubbed with the amalgam. They are faftened on two little boards of oiled wood, L M, L M, and from the faid cufhions proceed feveral wires of iron which join the bodies from which I intend to draw electrical fire: this apparatus will be found very convenient for charging glaffes. The two little boards L M, L M, are inferted into the table N N, which is faftened with fcrews to the beam Q. (Fig. 1.) The fpring R, ferves to make the cufhions prefs gently againft the plate.

52. Whenever globes or cylinders have contracted any confiderable dampnefs, it is impoffible to prevent the fire from diffufing itfelf from the rubbed zone, into the lateral armatures; and if the dampnefs lies in the infide of the globe, it is no eafy tafk to take it out; but a plate, on the contrary, is very eafily dried: befides, by its being rubbed on both its furfaces, it furnifhes a double quantity of electric fire, though we muft obferve that the refiftance from the friction becomes likewife double. There are fome perfons who ufe to rub the plate on two oppofite parts of it, with two pair of cufhions; but this contrivance produces the two bad effects of doubling the refiftance arifing from the revolving of the plate, and of heating it much fooner. Other perfons adjuft to one common axis, two, three, or even four plates of glafs; and to each of them feparate cufhions. Surely, if no inconveniency arofe from employing fuch a fuperior force as becomes neceffary for furmounting the refiftance from thofe multiplied frictions, it cannot be difputed but a given fum of electricity will thus be obtained in a time that will be proportionably fhorter; or a much larger fum of electricity in a given time. On the other hand, however, it will be difficult to make every one of thofe many plates retain fo well its fire as one will do; and it will be enough that one of them be not exactly infulated, to have the effect of all vanifh to nothing; befides it will be likewife very difficult to fit the rubbing bodies with a fame degree of exactnefs to every one of the plates as to one alone.

53. Many perfons reduce thofe larger apparatus of one or more plates of glafs, to two fupports that come out of a piece of board; upon thefe two fupports they fettle the axis of one or more plates, which revolve by the help of a handle; and they adjuft upon this

upon that ſame piece of boards the two cuſhions, and faſten it with ſcrews to a table. To ſay the truth, thoſe machines of ſo ſmall a bulk are extremely convenient, and eaſy to be tranſported about; but it is to be wiſhed they might be equally ſerviceable and really good. Without mentioning the difficulty of finding tables ſolid enough to faſten upon them thoſe apparatus, and that may be kept firm and free from agitation, notwithſtanding the motion of the latter; omitting likewiſe the inequality from the interrupted motion of the handle, I ſhall only obſerve, that in order to obtain the greateſt neceſſary effect, a velocity is requiſite much ſuperior to any one that a handle is able to communicate with a conſtancy: I own, that when the plate moves with a degree of velocity that exceeds certain limits, then the greateſt part of its margin flies from the contact of the cuſhions, which only can touch it in the more prominent parts of its ſurface [it is impoſſible to find and adjuſt a plate that may have all the parts of its ſurface within the ſame plan] and have not time to cloſe again towards the more depreſſed parts, and to preſs them; but, on the other hand, we muſt conſider, that whenever the plate leaves the cuſhions and ceaſes to be in contact with them, a part of the fire gathered upon its ſurface always gets back into them (as we ſhall preſently ſee), and that portion of fire which flows back thus, is the larger as the plate moves ſlower from the cuſhions; conſequently a common handle gives a velocity inferior to that which is requiſite for exciting either in the Chain, or the Machine, the greateſt effect of the electricity: all this is moreover conformable to experience, which is enough for our purpoſe, let the reaſon of it be what it will.

54. With regard to the Chain, or prime conductor, I have nothing here to add, except that it muſt be of an even poliſhed ſurface, that it may loſe the leſs its electricity. I ſhall add that I make uſe of a conductor, and Machine (in this I comprehend, as has been obſerved before, all the ſeveral pieces that belong to it) of a conſiderable ſurface, in order to render ſenſible in public the ſeveral ſigns of their contrary electricity. Laſtly, with regard to the inſulating of the chain, or prime conductor, I uſe ſtrings of organzine, diſpoſed as in *fe*, *fg*, *ih*, (Pl. I. Fig. 1.) and when I am to make experiments in uncertain weather, I fit to each of the ſtrings a little box of iron, filled with hot cinders.

CHAP. III.

In which is ſhewn the truth of the Franklinian theory, from the univerſal conformity of the ſame with the extenſive laws of unity, exiſtence, non-exiſtence, *and* connection, *to which the electrical ſigns, in the ordinary apparatus, are ſubject, as experience demonſtrates.*

55. THE eſſence of the Franklinian theory is this. 1ſt, The electrical fire is diffuſed in all bodies univerſally, and balanced there with itſelf. 2dly, And univerſally. If the natural proportion of electrical fire in bodies becomes altered, this fire ſpreads itſelf from the bodies in which it is thus become *ſuperabounding*, into thoſe bodies where it is become *wanting*, with a force proportioned both to its relative exceſs, and to its abſolute denſity; and in ſuch paſſage through the reſiſting medium, it produces electrical ſigns. 3dly, And in the ordinary apparatus, the glaſs, when rubbed, draws from the rubbing Machine its natural electrical fire; and in revolving, carries it to the chain, into which it diffuſes it, at that place to which it paſſes neareſt.

56. I now propoſe to ſhew that the experiments made with the ordinary apparatus, in whatever manner they may be made, and combined together, all agree with the ſaid theory above, as do alſo the general conſequences or laws reſulting from them; nay, manifeſtly require and claim this theory. And without loſs of time, I proceed to enumerate thoſe general and experimental laws, which, though they might be reduced to a leſs number, I ſhall, however, as I prefer clearneſs to brevity, carry to the number of ſeven. The firſt, I call it the law of *unity*; the ſecond, the law of *non-exiſtence*; the third, the law of *exiſtence*; the fourth, I ſhall name it the law of *connection*; I ſhall give the appellation of law of *proportioned diſtribution* to the fifth; of *indication* to the ſixth; and laſtly, I ſhall deſign the ſeventh by the name of law of *excitation*.

57. And in order ſucceſſively to demonſtrate the univerſality of all theſe laws, I ſhall begin with that of *unity*; that is, I ſhall begin with ſhowing that *all the vivid ſigns of electricity are produced by a ſame identical electrical fire, which exerts itſelf after a ſame conſtant manner.*

58. In

58. In fact, a given quantity of electricity, either in the Chain, or the Machine, is indifferently lessened or annihilated, whether by sparks, or by motions, or by a wind; or partly by sparks and partly by motions; or partly by motions and partly by a wind; or partly by a wind and partly by sparks; or wholly by sparks, or wholly by motions, or wholly by a wind.

59. In every one of those signs the electrical fire may be said really to manifest itself. If, standing in the dark, I try to lessen a given quantity of electricity by the means of a wind, I then see the electrical fire sparkle upon the point from which the wind blows, under the shape of a *brush*, or a *little star*. If I try to lessen the electricity by the help of the *motion* of a light suspended body, I see a little spark shine between the Chain or the Machine, and the little pendulum; or between the little pendulum and my finger.

60. However, in order to annihilate or suppress a given quantity of electricity, by the means of signs of a given kind, a greater number of these is required in proportion as a less quantity of the electrical fire is actuated and manifested in every one of them. Thus, in order to suppress a given electricity, a single spark, which I excite by presenting my finger to the electrified body, may happen to be sufficient, while one hundred vibrations of a small pendulum, which will excite perhaps one hundred sparks, small and continually decreasing, will prove insufficient. The wind will lessen the same electricity with a much greater celerity; but at the same time a light will take place, much superior to that excited by the pendulum.

61. All that we say here with respect to the destroying or lessening a given electricity, obtains also, as we shall see after, with respect to producing the same; that is, we shall see that an electricity may be produced by all the abovementioned signs, indifferently; and that a greater number of these signs will be required, as a less quantity of electrical fire will happen to be manifested in them. Whence it plainly appears that such signs may, in all cases, be substituted to each other indifferently, in order either to produce or destroy a given quantity of electricity; and that, whatever may be the kind of the sign which is made use of, a given sum of motion in the electric fire must take place, for producing the desired

 effect.

effect. Therefore, we muſt conclude that electrical ſigns are, in all caſes, produced by an identical electrical fire, moving itſelf in a ſame conſtant manner; and in this conſiſts the firſt law of unity. The abovementioned ſigns are likewiſe ſubject to the general laws expreſſed above, of *non-exiſtence*, of *exiſtence*, *connection*, &c. For the explanation of which laws we have prepared the way, in beginning with the firſt.

62. To proceed therefore to the law of *non-exiſtence*; the firſt part of that law is, that *there never are electrical ſigns between two parts of one ſame ſyſtem*, whether ſuch ſyſtem be an *indifferent* ſyſtem, or one of the two ſyſtems which I call *animated* ſyſtems. In the ordinary apparatus, I call the glaſs an *animating ſyſtem*, becauſe it draws, when rubbed, the fire from the Machine, and, in revolving, carries it to the Chain, to which it imparts it: on account of thoſe functions it therefore may be ſaid to *animate* the electricity in the Machine negatively, and in the Chain poſitively: I reſerve for the concluſion to diſcuſs this ſyſtem, and ſhew in what manner it performs its functions. On the other hand, I call *animated ſyſtems* the machine, and the chain: as to the floor, or bodies that communicate with it, I comprehend them under the appellation of the *ſtrange* or *indifferent* ſyſtem.

63. That between two parts of ſuch indifferent ſyſtem, no electrical ſigns ever are perceived, is a truth founded on conſtant experience: a ſpark never was ſeen to riſe, or a light to appear, between the fingers of two men who, communicating with the ground, ſhook hands together. Such a *non-exiſtence* of ſigns is a neceſſary conſequence of the theory above: the electrical fire cannot be diffuſed in *indifferent* bodies without being diffuſed equally; or, at leaſt, in proportion with the reſpective electrical capacities of theſe bodies if they are *deferent*, and communicate among themſelves.

64. Hence, perſons little uſed to electrical experiments do not fail to be aſtoniſhed when they ſee two men, both inſulated and communicating with the electrified Chain, excite ſtrong ſparks whenever they approach their fingers to ſome ſtrange body, while they are unable to excite the ſmalleſt one by preſenting them to each other: or when they ſee them agitate ſuch corpuſcula as are preſented to them by ſtrange bodies, and be abſolutely unable to

communicate any ſuch agitation to any one they may offer to each other.

65. Indeed this *non-exiſtence* of ſigns between the various parts of a ſame ſyſtem after whatever manner it may have been *animated*, follows likewiſe from the ſame theory: there is no reaſon why the electrical fire ſhould paſs from one part of the chain to the other, while, through the *deferency* of the ſubſtance of the latter, this fire is equally diffuſed within all its parts; and if ſuperabounding in one, is neceſſarily balanced by an equal ſuperabundance in all the others. The ſame may be ſaid of the machine, however wanting or defective in electrical fire it may be; the fire remaining in one part is balanced by that which alſo remains in the other.

66. It is true, that if we uſe as a Chain, a body made of parts imperfectly united, viz. a real chain of iron, eſpecially if ruſty, we ſhall ſee when this chain is electrified, or when ſparks are drawn from it, other ſparks leap between the links, where they touch each other imperfectly and in few points; but this obſervation only ſerves to ſhew that the ſuperabounding fire, which in the ſaid chain proceeds from the globe, endeavours, conformably to the theory, equally to ſpread itſelf; and when effecting this, ſparkles at every little reſiſtance it finds in its way, and conſequently at every ſeparation between the links: this obſervation does by no means confirm an opinion, that the fire which is equally diffuſed in a body will not move from one part of it to the other. However, in order to avoid this ſeeming irregularity, I uſe as a Chain a conductor of a perfectly continued ſurface. Likewiſe, I uſe for Machines an aggregation of bodies, as continued and as even as can be procured, and all ſufficiently deferent. I uſed at firſt to have the wood of the machine gilt; but I have ſince found it better to clothe it, at leaſt the more outward parts, with tin.

67. It muſt be added, that from the ſame ſecond principle, viz. that ſigns do not exiſt between two parts of a ſame ſyſtem, it alſo follows that theſe ſigns will not exiſt, neither between an *animated* ſyſtem, if it is not inſulated, and ſtrange bodies. The reaſon is, that the animated ſyſtem will, in that caſe, be itſelf one of the ſtrange bodies. Thus if the Chain communicates with the floor, a certain quantity of fire will indeed diffuſe itſelf into it from the glaſs;

glaſs; but then an equal quantity of it will run from the other extremity of the Chain into the floor. Likewiſe, a quantity of fire equal to that drawn by the glaſs from the machine, will be ſupplied from the floor into the ſame, through that part where their mutual communication takes place; thence it follows that the natural quantity of fire continues in the ſaid ſyſtems, though electrified, provided they are not inſulated. We might, if it were neceſſary, confirm that explication, by making thoſe ſyſtems communicate leſs perfectly with the floor, for inſtance through a well dried flaxen thread; and in ſuch caſe, we would ſee ſigns ariſe from ſuch obſtacles as the exceſſive fire would then meet in its way from the Chain into the floor; or the *natural* fire in its way from the floor into the Machine, which, when the electricity is excited with glaſs, is in a deficient ſtate. Signs, indeed, will be perceived between the Chain not inſulated, and the Machine if inſulated; or between the Machine not inſulated, and the Chain if inſulated; but, in ſuch caſes, either the Chain, or the Machine, muſt be looked upon as being a part of the floor; conſequently, ſuch ſigns muſt be conſidered as ſigns between the *animated* inſulated body, and the *indifferent* ſyſtem.

68. The other part of the law of *non-exiſtence* is that, *if two animated ſyſtems, that is the Machine and the Chain, communicate with each other, they manifeſt no electrical ſigns, neither between themſelves, nor with regard to ſtrange bodies, however inſulated theſe two animated ſyſtems may be.* The reaſon is, that in ſuch caſe the rubbed glaſs will indeed draw ſome electrical fire from the Machine, and tranſport it into the Chain, but as that fire is enabled, in conſequence of the communication, to run back from the Chain into the Machine, it can neither become deficient in the one, nor *exceſſive* in the other.

69. Let us proceed now to the explanation of the third law, which I have named the law of *exiſtence.* The firſt part of it is, that *if the Chain be inſulated, and the Machine at the ſame time communicates with the floor, perpetual electrical ſigns will take place in the Chain.* Becauſe, in ſuch caſe, the fire which the glaſs draws from the Machine, is allowed to accumulate itſelf to a certain degree in the Chain; and as the Machine from which the glaſs draws its fire

by

by its communicating with the ground, becomes in a manner a part of it, and has confequently an infinite electrical capacity, the glafs will continually be fupplied with new fire from it: therefore any ftrange body that has only its natural quantity of electrical fire, will be able continually to draw fire from the Chain. Though I fay continually, this muft however be underftood with the referve that fuch continual extraction of the fire does not actually deftroy the accumulation of it, or the infulation neceffary to it. If I keep, for inftance, my finger at the diftance of two thirds of an inch, or an inch from the chain, I fhall obtain fparks from it without end; that is to fay, as long as the rubbing of the glafs continues, and neither heat, or other accident difables it from performing its function. Thofe fparks, however, will be feparated from each other by fuch an interval of time as will be requifite to enable the glafs to tranfport into the conductor, the *excefs* of fire neceffary for furmounting the refifting fpace of either the two thirds of an inch, or of an inch. If after bringing your finger to a contact with the Chain, and keeping it united to it, you pretend to obtain farther figns, it will be a vain pretenfion, fince you have deftroyed beforehand the neceffary condition of the exiftence of figns, that is the infulation.

70. It will not be thought improper here to obferve that we may compare the ftrength of different apparatus, by holding a given ftrange body at a given diftance from fimilar parts of thefe apparatus, as I have done juft now with my finger. According as the number of the fparks that iffue in a given time from a given conductor, is greater, the fire fupplied by the glafs muft be concluded to be more copious, and the apparatus to which it belongs more excellent.

71. I muft add, that from the preceding obfervation we may infer which is the beft way to obtain the greateft figns, that is, the greateft fparks, from a given conductor. When a fpark has been drawn, the body which drew it muft be removed, and not approach again till the conductor be charged afrefh to the greateft poffible excefs it is capable to receive.

72. The other part of the law of *exiftence* regards the Machine, and refembles the firft: that is, *if the Machine is infulated, and the*

Chain

Chain communicates with the ground, there will be perpetual ſigns between the ground and the Machine; the reaſon is, becauſe a ſame quantity as that which the glaſs draws from the Machine, will be diffuſed through the Chain, which communicates with the ground, into the infinite capacity of the latter: conſequently, no fire will be accumulated and forced back into the Machine or the glaſs, and prevent the latter from extracting freſh fire from the Machine; ſo that the glaſs will in ſuch caſe be able to extract whatever fire may remain in the Machine, and whatever quantity may at due intervals of time be diffuſed into it (it being in a wanting ſtate) from any ſtrange body that has the natural proportion of electrical fire.

73. Thoſe two firſt parts of the law of *exiſtence* ſuppoſed that the one of the animated ſyſtems communicated with the ground, and from that hypotheſis there muſt reſult a perpetuity of ſigns between the other animated ſyſtem and the ground; now the third part of this ſame law will ſuppoſe that both animated ſyſtems are inſulated, and is ſubdivided into two other parts, according as the ſigns are excited between the ſaid ſyſtems, or either of them and ſtrange bodies.

74. Therefore, with regard to the firſt caſe I ſay, *that if the Machine and the Chain be both of them inſulated, there will be between them perpetual ſigns.* I inſulate myſelf, and with one of my hands I touch conſtantly the inſulated Chain; at diſtant intervals of time I then preſent my other hand to a perſon who touches the inſulated Machine; and the reſult is, that I am continually giving him ſparks after ſparks. This is becauſe every ſpark that I give is, in fact, a little ſum of fire, which the glaſs can extract anew from the machine; the ſame fire can anew be accumulated into the Chain of which I am a part; and I can diffuſe it anew into the Machine, which alſo is exhauſted anew.

75. Let us retain the ſame hypotheſis, and continue to ſuppoſe that the Chain and the Machine are both inſulated; and inſtead of examining, as in the preceding caſe, the ſigns that take place between thoſe ſyſtems, let us conſider the ſigns that may ariſe between one of them, and ſtrange bodies. But here ariſes another ſubdiviſion, that is to ſay, the Machine and the Chain either are tried together, or are not: if it be the firſt caſe, I ſay, that *there*

will

will be perpetual ſigns between the ſtrange bodies and the Chain, and between the ſtrange bodies and the Machine. In fact, if while I try the Chain with one hand, and in conſequence draw from it the fire that the glaſs has tranſported into it from the Machine, I at the ſame time try the Machine with my other hand, I then return to the latter the fire of which the glaſs has robbed it: I alſo give to it a quantity of fire; which quantity the glaſs will again take from it and diffuſe into the Chain: I then ſhall always have an exceſs to take from the Chain, or a deficiency to ſupply in the Machine.

76. But if the Machine and Chain continuing inſulated, I try only one of theſe two ſyſtems, *I then ſhall only obtain very few ſigns, and theſe will be proportioned to the capacity of the other ſyſtem.* Thus, if I determine to try the Chain alone, I ſhall only obtain one, two, or at moſt three ſparks, and thoſe continually decreaſing; becauſe I ſhall then be able to extract from the inſulated Chain but as much fire as the glaſs ſhall have been able to tranſport from the Machine into it. Likewiſe, if I determine to try the Machine alone, I ſhall then produce only ſome little decreaſing ſparks; becauſe in this caſe I ſhall not be able to ſend into it but juſt a quantity of fire equal to that which the glaſs ſhall have been able to take from it and carry to the Chain.

77. Farther, if after having exhauſted the electricity of one of the animated ſyſtems, I paſs to the other and try it, *I ſhall have the ſame ſigns, ſtill decreaſing, but then a greater ſum of them,* that is, a ſum proportioned to the capacity of the two ſyſtems taken together. Thus, if after having exhauſted firſt the electricity of the inſulated Chain, I paſs to the Machine, I ſhall then be able to give to it a quantity of fire equal, both to that which the glaſs can again tranſport from it into the Chain, and to that which is neceſſary to make up the deficiency introduced before into it. Likewiſe, if after having deſtroyed firſt the electricity of the Machine, I paſs to the Chain, I ſhall find myſelf able to draw from it a quantity of fire equal both to the exceſs already introduced in it, and to the deficiency which the glaſs can again create in the Machine. Laſtly, if both ſyſtems are alternately tried, by the ſame reaſon, ſigns will be alternately obtained, which will firſt decreaſe, then fail entirely.

78. In repeating theſe experiments I have ſought to increaſe conſiderably the capacity of either, or both of the animated ſyſtems, by adding to them a ſeries of inſulated men; and I have always ſeen the conſequence to be, that the ſigns from the one of the ſyſtems always increaſed with the increaſe of the capacity of the other; and when I purſued the experiment, that the ſigns from the other ſyſtem increaſed with the increaſe of the capacity of both.

79. Indeed that law demonſtrates beyond a doubt, that *in all bodies a certain quantity of electrical fire is naturally diffuſed; and that to any body whatever a certain quantity of electrical fire may be added.* A man A is inſulated in ſuch a ſituation that he may with one of his arms reach the inſulated Machine; and with a rod of braſs which he holds in the other, touch a pail of water, or any other body alſo inſulated: this done, after having drawn from the Chain all the ſparks I can draw from it, I bid the man to touch the Machine; and the conſequence is that I then can draw from the Chain another ſpark, and no more; which ſpark is no other than the fire contained in the man A, and which the glaſs can tranſport from him into the Chain, he being become a part of the Machine. I make again a ſign to the man to touch now the pail of water with his rod; as the pail is then become a part of the Machine, I obtain from the Chain another ſpark, in conſequence of the new fire which the glaſs has extracted from the water, or any other body that may have been uſed in its ſtead.

80. I diſpoſe things ſo as to have the man A inſulated in ſuch a manner that he may touch on the one hand the inſulated Chain, on the other the pail of water or any other inſulated body; then I give to the Machine the fire that is neceſſary to make up its deficiency, and till it receives no more. But as ſoon as the man touches the Chain, then the Machine draws another ſpark and no more: when the man ſtretches the rod to the pail of water, the Machine becomes again able to receive from me another ſpark, all which I explain thus; when the man communicates with the Chain, the exceſs of it diffuſes itſelf for a part into him; and when the man introduces likewiſe a communication between the Chain and the pail of water, the exceſs in both the Chain and the man, is again divided and diffuſed into the water; ſo that, in theſe two

caſes,

cases, the glass will be able to draw from the Machine, and transport into the Chain, a proportionally larger quantity of fire.

81. All these things, on which I have hitherto pretty much expatiated, indeed comprehend the whole substance of what I am going to say concerning the law that I have called of *connection*; but I think that it will not be deemed superfluous to confirm those truths by considering them under different relations, and examining them through other experiments. The law of connection is therefore, that *absolute electrical signs lessen absolute electrical signs of the same name, but increase absolute electrical signs of a contrary name; and that respective signs diminish both respective and absolute signs.* When I introduce the terms *absolute* signs, and *respective* signs, I have no other reason but to expose the law with less words; and by the words *absolute* signs, I mean those that are excited between the ground, and one of the animated systems: by *respective* signs, I mean those that are excited between the two animated systems. Again, I call *absolute signs of a same name*, those that are excited between the ground and the Chain, when they come to be compared with others excited in the same manner; or those excited between the ground and the Machine, when compared with those excited between the ground and the Machine. On the other hand, I call *signs of a contrary name*, those that are excited between the ground and the Chain, with respect to those excited between the ground and the Machine.

82. I express the whole of the law above by a single experiment: I adapt two very sensible electroscopes, the one to the Chain, the other to the Machine, which, as the law expresses it, must be both insulated. I say, two very sensible electrescopes, because if they are made as usual, of thread, though they are ever so mobile on account of their lightness, however in dry weather they stand almost without motion, and instead of moving towards each other when I lessen the electricity which they are destined to indicate, they continue to recede from each other: the reason of this is, that their electricity does not by any means keep pace in its decrease with the electricity in the Chain, owing to their being in a great degree insulated from the dryness of the weather; which circumstance makes them to manifest the decrease of the electri-

city either in the Chain or the Machine, but after an interval of time proportioned to the difficulty they find in diſſipating their own. Therefore, in that caſe and others of the ſame kind, inſtead of the threads and balls, I uſe two ſtripes of tinfoil, two or three inches long, and two lines wide, which are fixed to a band of gilt paper, and thus hang contiguous and parallel to each other.

83. And to proceed to the experiment itſelf. 1ſt. I obſerve that as ſoon as I draw a ſpark from the Chain, the divergency of the metallic ſtripe that communicates with it leſſens, and that of thoſe annexed to the Machine is increaſed. 2. When I give ſparks to the Machine, the ſtripes that communicate with it loſe their divergence; thoſe annexed to the Chain recover theirs: now, the manner of the divergence of the metallic ſtripes ſhews the kind of the electricity of a given ſyſtem, or otherwiſe, what ſigns can be obtained from it? conſequently, we may ſay that the ſigns between the Chain and the ground, leſſen the ſigns between that ſame Chain and the ground, and increaſe thoſe between the ground and the Machine: and, in the ſame manner, the ſigns between the Machine and the ground leſſen thoſe between that ſame Machine and the ground, and increaſe thoſe between the ground and the Chain. The reaſon of all is evident, and follows from what has been ſaid with reſpect to the laws expreſſed before: to draw ſigns from the Chain is to diminiſh its exceſs, and create in the ſaid Chain a vacancy in which the glaſs may again tranſport a quantity of natural fire from the Machine; whence there reſults in the latter an additional deficiency. To create ſigns in the Machine, is the ſame as to ſupply it with a new fire, which the glaſs may again tranſport into the Chain, there to produce a new exceſs.

84. And, to ſpeak the truth, we have with regard to deſtroying homologous ſigns by one another, more examples and confirmations than might be well wiſhed for: when perſons little converſant with electrical operations come to ſee our experiments, being in conſequence the more curious and eager, they handle every thing, and take among themſelves ſo much trouble that they at laſt ſee nothing.

85. With reſpect to *reſpective* ſigns, the truth of the law is no leſs beyond a doubt. I inſulate myſelf and communicate with the Chain;

Chain; at every ſpark I then give to another perſon likewiſe inſulated, and communicating with the Machine, the two electroſcopes, that annexed to the Chain, and that annexed to the Machine, both fall down: a diminution therefore takes place then of the ſigns between the Chain and ſtrange bodies, or between ſtrange bodies and the Machine, that is to ſay, of the *abſolute* ſigns; and there ariſes likewiſe a diminution of the ſigns between the Chain and the Machine, that is to ſay, of the reſpective ſigns. In fact, if in the ſame moment that I give the ſpark, the friction of the glaſs is interrupted, I cannot give a freſh ſpark, or if I ſtill can, it will be leſs; it is, therefore, a matter of truth, that if we conſider its function the glaſs may be ſaid to begin again the circulation of the fire, but it is the *reſpective* ſigns that complete it.

C H A P. IV.

On the law of diſtribution.

86. THOUGH this law is cloſely connected with the ſubſtance of thoſe explained above, however, I will explain it ſeparately in this chapter, as a thing that ſerves to prove, in a particular manner, the conformity of the theory with facts; a means, this, of attaining truth indiſpenſible to philoſophers, and the beſt guide they can have for recovering it when loſt. The purport therefore of the law is, *that the ſum of any ſort of abſolute electricity in any two bodies, diſtributes itſelf in them proportionably to their capacity, as ſoon as they come to communicate with each other.* I ſhall expoſe firſt all the particular caſes belonging to that law; then I will proceed to ſhew, as far as it will relate to the ſubject, its general and exact conformity with experiments.

87. The natural denſity of the electric fire, as it is diffuſed in the ground, is the circumſtance from which we form our calculations of the quantity of electricity in bodies. Bodies in which the electrical fire is equally denſe with that in the ground, afford no ſign of electricity; and thoſe in which it has been rendered more or leſs denſe afford ſigns, or at leaſt are in a condition of procuring them, whenever they ſhall communicate with the ground.

88. There-

88. Therefore, any ſort of *abſolute* electricity is formed either by an exceſs of fire by which the fire of the given body is more denſe than that in the ground, and is conſequently called electricity by exceſs; or by a deficiency of the ſame, which makes the fire in the given body to be leſs denſe than that in the ground, and is then called, electricity by deficiency. Therefore, if we multiply the exceſs of denſity in a given body relatively to that in the ground, by the capacity of the body itſelf, the produce will be the quantity of its electricity by exceſs, or alſo, the quantity by which the whole of its fire is denſer than that in the ground: and likewiſe, if we multiply the deficiency of denſity in a given body, relatively to that of the fire in the ground, by the capacity of the ſaid body, the produce will be the quantity of its electricity by deficiency, or the quantity of the deficiency by which it is leſs denſe than the fire in the ground.

89. Hence if E expreſſes the exceſs of denſity of the fire in a given body, of the capacity A; A E will expreſs the whole quantity of the exceſſive fire from which reſults the exceſs of denſity; and if S expreſſes the capacity of the ground, the following analogy will be the conſequence of the abovementioned rule. $A+S : AE = A : \frac{AAE}{A+S} = S : \frac{SAE}{A+S}$; that is, as the ſum of the capacities is to the whole exceſs, ſo are the capacities of the body, or of the ground, to the portions of the exceſs that belong to them: that is to ſay, the portion of exceſs that will remain in A, will be to that portion which will paſs into the ground, as A : S. Now, as the capacity of the body A is infinitely ſmall, relatively to the capacity of the ground, all the ſum of the exceſs A E will paſs from A into S, and during ſuch paſſage, ſigns will be manifeſted, which will be proportioned to the whole above mentioned ſum: therefore, that ſum will expreſs the preciſe quantity of the electricity.

90. Likewiſe if D expreſſes the deficiency of denſity in the fire of a given body of a capacity B, B D will expreſs the whole quantity of fire that forms the deficiency of denſity in A. And in forming the ſame analogy as above, we ſhall ſee, that the portion of deficiency B D which will remain in the body, will be to the portion that will paſs into the ground as B : S, that is to ſay, no part of the

the deficiency B D will remain in the body B, all will paſs into the ground, it being of an infinite capacity: or rather the ground will ſupply the body with what it is wanting of, ſo that the fire in it may be rendered of an equal denſity to that of the fire in the ground.

91. In ſhort, when a body having any degree whatever of electricity, either per exceſs A E, or per deficiency B D, is made to communicate with the ground, all manner of electricity is ſuppreſſed; the whole exceſs, or the whole deficiency paſſes into the ground; tho' no perceivable electricity reſults in the ground from its receiving ſuch finite quantities of electricity into its infinite capacity.

92. But the ſame does not hold true, when a communication is eſtabliſhed between two particular bodies, which have, either one or both of them, any degree of abſolute electricity. In this caſe, the fire that is diſtributed in both in an unequal proportion will, indeed, be brought to an equal denſity, and the electrical ſigns from the one to the other be ſuppreſſed; but their fire is not, for all that, brought to the ſame degree of denſity with the fire in the ground, nor is conſequently their abſolute electricity deſtroyed.

93. Therefore, that we may be able to diſcern in all caſes the electrical ſigns that may take place between any two particular bodies whatever, as well as thoſe between a particular body and the ground, it is requiſite to divide not only the abſolute, but likewiſe the reſpective kinds of electricity; that is, thoſe which may exiſt in a particular body, relatively to another, and which conſiſt of the quantity of fire that muſt paſs from the one into the other for bringing them to a ſtate of equal denſity. Thoſe kinds of reſpective electricity will be, 1ſt, a reſpective electricity of ſimple exceſs, when a particular body A, having a degree of abſolute electricity per exceſs, is made to communicate with another particular body that has no electricity. 2dly, Reſpective electricity of ſimple deficiency, when a particular body A, having a degree of electricity by deficiency, is brought into a communication with another body B, that has no abſolute electricity. 3dly, Reſpective electricity of unequal exceſs, when the communication is eſtabliſhed between two bodies, having both a degree of abſolute electricity by exceſs, but

thoſe degrees are unequal. 4thly, Reſpective electricity of unequal deficiency, when the two bodies between which the communication is formed, are both electrified by deficiency, but in unequal degrees. 5thly, Laſtly, reſpective electricity by exceſs and deficiency together, when the one of the two bodies that are brought to communicate together has an abſolute electricity by exceſs, and the other by deficiency.

94. The degree, whatever it may be, of each of thoſe kinds of reſpective electricity, is expreſſed in the following table, in which the quantity of fire that conſtitutes within the capacities A and B, their different denſity, is diſtributed in proportion to the ſaid capacities; which quantity of fire, in caſes of ſimilar kinds of abſolute electricity is equal to their difference; and in the caſe of contrary kinds of abſolute electricity, is equal to their ſum. E and D expreſs the exceſs or deficiency of denſity in the fire. And *e*, *d*, will expreſs another degree of exceſs or deficiency in the denſity.

	I.	II.	III.	IV.	V.	VI.
95.	As the ſum of the capacities	is to the difference of the quantities of ſimilar electricities, and to the quantities of contrary eleccities.	So the capacity A	is to the difference or ſum that remains in A	So the capacity B	is to the other portion that paſſes into B, and which conſtitutes the quantity of the reſpective electricity.
96.		AE		$\frac{AAE}{A+B}$		$\frac{BAE}{A+B}$
97.		AD		$\frac{AAD}{A+B}$		$\frac{BAD}{A+B}$
98.	$A+B$	$AE-Be$	$=A$	$\frac{AAE-ABe}{A+B}$	$=B$	$\frac{BAE-BBd}{A+B}$
99.		$AD-Bd$		$\frac{AAD-ABd}{A+B}$		$\frac{BAD-BBd}{A+B}$
100.		$AE+BD$		$\frac{AAE+ABD}{A+B}$		$\frac{BAE+BBD}{A+B}$

101. The

101. The quantity of a refpective electricity in the cafe of a fimple excefs, or alfo of a fimple deficiency, is lefs than that of an abfolute electricity; that is to fay, the quantity of the fire A E, or A D, that will pafs from the body A into the ground, or from the ground into the body A, if this comes to communicate with it, is to the quantity $\frac{BAE}{A+B}$, or to the quantity $\frac{BAD}{A+B}$, when A comes to communicate with B, as the fum of the capacities A+B, is to the capacity B (VI. 96. 97.)

102. The refpective electricity, in the cafe of an unequal excefs, or of an unequal deficiency, is lefs than the refpective electricity of fimple excefs, or of fimple deficiency; fince the former is to the latter, as the difference alone of the abfolute electricities, (from which the former refults) is to the whole of the abfolute electricities from which the latter refults.

103. But the refpective electricity by excefs and deficiency together, (the capacities are fuppofed conftant) being proportioned to the fum of the abfolute electricities (100.) will always be greater than the leaft abfolute electricity, though always lefs than the greateft, whenever the abfolute electricities will happen to be unequal; and will be equal to them, whenever they will happen to be equal. 1ft, Let the abfolute *excefs* A E be equal to the abfolute deficiency B D; if the former A E paffes into the latter B D, or the former into the latter, which comes to the fame, thofe parts of their values that will be equal and contrary, will deftroy each other. 2dly, Let A E be greater than B D, there will firft pafs into B, as great a portion of A E as is neceffary to deftroy the whole B D; and that portion of refpective electricity muft be equal to the leaft abfolute electricity; but, moreover, the overplus in A E above the deftroyed quantity B D, will diffufe itfelf into A and B, proportionably to their refpective capacities; in confequence of which diffufion, the refpective electricity will indeed be lefs than the greateft abfolute electricity A D, but greater than the leaft abfolute one B D. 3dly, The fame is to be faid in the cafe where the abfolute electricity A E fhould be lefs than the abfolute one B D; the greateft refpective electricity will be lefs than the greateft abfolute one B D was, but greater than the leaft abfolute one A E, by all the overplus in B D, above the value

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of A E; after the deſtroying of which quantity A E the ſaid reſpective electricity will diffuſe itſelf into the capacities A and B, proportionably to their reſpective amounts.

104. Whence it follows, that in the caſe of a reſpective electricity by exceſs and deficiency together, the abſolute electricities either deſtroy each other, if they are equal, or the leaſt one, in conſequence of the communication between the two bodies, becomes of a contrary nature, and differs in that reſpect from the four other ſorts of reſpective electricities; viz. thoſe of ſimple exceſs, or deficiency, and of unequal exceſſes or deficiencies; in which reſpective electricities the remaining abſolute ones both retain the ſame poſitive, or negative value of the primitive abſolute electricities.

105. But let us now proceed to the experiments. Four men, A, B, C, D, are inſulated in a proper poſition, each of them holds a very ſenſible electroſcope between the index and thumb of his left hand; A touches the Chain with his right hand and then withdraws it; B touches the left hand of A with his right hand, which he then likewiſe withdraws; then C touches B, and laſtly D touches C. In the ſame time that thoſe ſeveral ſucceſſive contacts are performed, the divergence of the electreſcope held by the man who is touched, is leſſened by a half part of it, and a divergence equal to the remaining part, riſes in the electreſcope of the man who has touched him. I afterwards touch from the floor, the men A, B, C, ſucceſſively, when I draw from each of them ſparks which prove ſucceſſively dimidiated. Thus that experiment demonſtrates, by the help of the regular diminution in the divergences and ſparks, how the electricities of ſimple exceſs of different degrees, are diffuſed and diſtributed.

106. If the men A, B, C, ſtand with their reſpective degrees of electricity after the firſt operation, mentioned in the preceding paragraph, then A will retain $\frac{1}{2}$ (or $\frac{8}{16}$) B $\frac{1}{4}$ (or $\frac{4}{16}$) C $\frac{1}{8}$ (or $\frac{2}{16}$) of the whole electricity which A had drawn at firſt from the Chain; and D, who has not been touched by any other ſubſequent perſon, will have $\frac{1}{8}$ or $\frac{2}{16}$ of the ſame. Now, if in ſuch a ſtate of things, A touches B, he will give him a ſpark equal to $\frac{2}{16}$ of the former total electricity, and conſequently ſhall retain $\frac{8}{16} - \frac{2}{16} =$ to $\frac{4}{16} + \frac{2}{16}$, the value now in the man B. But if in the ſame former ſtate of things,

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A immediately touches C or D, inftead of B, he will give to either of them a greater fpark, which will be equal to $\frac{3}{16}$ of the original fpark; thus $\frac{8}{16}-\frac{3}{16}$ will be equal to $\frac{2}{16}+\frac{3}{16}$. In this cafe, likewife, it will be convenient to ufe the electrofcopes; and the agreement that will be obferved between both the fucceffive values of the fparks and the changes in the divergencies, on the one hand, and the reafoning that I have expofed above on the other, will demonftrate the truth of the theory of the electricity of unequal excefs, from which that fame reafoning is deduced. Nay, fince fuch experiments, and others of a fame kind may be repeated at pleafure, and be always attended with the fame effects, (referved the contrariety of the direction with regard to the Machine) they can likewife manifeft how much refpective electricity of fimple excefs, and electricity of unequal deficiency, has been introduced.

107. The Machine and Chain are infulated; the men A and B, are likewife infulated in a proper fituation, and fupplied with electrofcopes; I touch the Machine, and the man B touches the Chain; then I leave the Machine and touch the Chain, and the man B touches the Machine. The electrofcopes of A and B, diverge equally in confequence of equal but contrary electricities; the fame friction of the glafs being able to introduce from the ground into the man A, the fame quantity of fire as that which it may extract, and diffufe into the ground from the man B, who is fuppofed of an equal capacity with the other. The man A touches the man B, and the electrefcopes immediately lofe all manner of divergency; which fhews how abfolute electricities, when equal and contrary, reciprocally deftroy each other.

108. The man A ftands infulated alone, and alternately touches the Machine and the Chain, which are infulated. At every fucceffive contact the electrofcope which he holds between his fingers falls down, and then rifes and diverges again; which fhews, that at every contact the electricity in the man A, turns into one of a contrary nature. The reafon is, that the excefs which the man A, who is of a capacity inferior to that of the Machine, draws from the Chain, is lefs than the deficiency that he receives from the Machine, which is, as we obferved, of a fuperior capacity to his; and the deficiency that he may contract from his touching the

Machine is lefs than the excefs he will receive from his touching the Chain, which is likewife of a capacity fuperior to his: which experiment will fhew how the refpective electricity per excefs and deficiency together, transforms the leaft abfolute electricity into the nature of the greateft abfolute one.

109. If after the man A, in the preceding experiment, has touched the Chain, I touch both the Chain and the man, the fpark from the man is lefs than that from the Chain: likewife, if after the man touched the Machine, I touch both the man and the Machine, the fpark that I give to the man is lefs than that I give to the Machine. This proves how the transformed electricity remains lefs than the greateft transforming electricity.

110. But we muft here obferve, that at every touching of one of the animated fyftems, the divergency of the electrofcope of the man becomes equal to the divergency of the fellow-electrofcope which is annexed to the fyftem; we may even lay it down in the moft univerfal terms, that as foon as the quantities, whatever they are, of electricity, in two bodies of capacities however unequal, have been diffufed into each other, by the means of a communication, or, as foon as the fire becomes diffufed in the fame bodies in an equal degree of denfity, whether pofitive or negative, the electrofcopes always are found to have an equal divergence, let the quantity of the fire, either exceffive or refidual diffufed in both be in what degree it will; which is owing to that fire being always diffufed in thefe bodies proportionably to their capacities. Whence it follows univerfally, that the exceffive or defective denfity of the fire is always manifefted by the divergence of the electrofcope; but the fame cannot be univerfally faid of the *quantity*. 2dly, The divergence manifefts the quantity only when the bodies amongft which the fire has been diffufed, are of an equal capacity, as was the cafe in the experiment of the four men infulated, who are to be fuppofed to be of an equal capacity; in which cafe the quantity, I fay, is proportioned to the denfity.

111. Here a queftion can be propofed: in the refpective electricity of unequal excefs and deficiency, the fame quantity of fire indeed paffes from the one of the particular bodies into the other, as would do in the cafe of an abfolute electricity; but will there be

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no difference in the manner after which the fparks will then be produced?—In comparing the fparks which, in the cafe of the experiment N° 107, take place between the man A and the man B, it appeared to me that thefe fparks were more united, and leaped to a diftance fomewhat greater, and produced cracks fenfibly louder, than thofe fparks did, which either of them gave or received from the floor: in both cafes the fame quantity of fire indeed paffed, but the fire in the cafe of the refpective electricity, feemed to me to have more impetuofity, and to be more compact. The fame appeared to me ftill more evidently, when I compared the fparks between two men, who communicated, the one with the Chain, the other with the Machine and were conftantly infulated, with the fparks that one of the men either gave or received from the floor. The fame has been confirmed by Monf. Le Roy, in the volume of the *Memoires de l'Académie Roiale des Sciences, de l'année* 1753, publifhed in the year 1757, by exciting the fparks with a ball of brafs, which, by the help of a rod annexed to it, was moved backwards and forwards within the infide of a tube of glafs, of which the mouth refted on the body from which the faid gentleman intended to draw the fparks. All this is conformable to the theory; a given fum of electrical fire muft diffufe itfelf with more impetuofity from the Chain within which there is an excefs of it, into the Machine where the fame is deficient, than from the faid Chain into the floor, which has the natural proportion of it. The fire muft likewife have a greater impetuofity when diffufing itfelf from the floor, which has the natural proportion, into the Machine, which is deficient; all this will be proved by moft evident experiments in the next chapter.

CHAP. V.

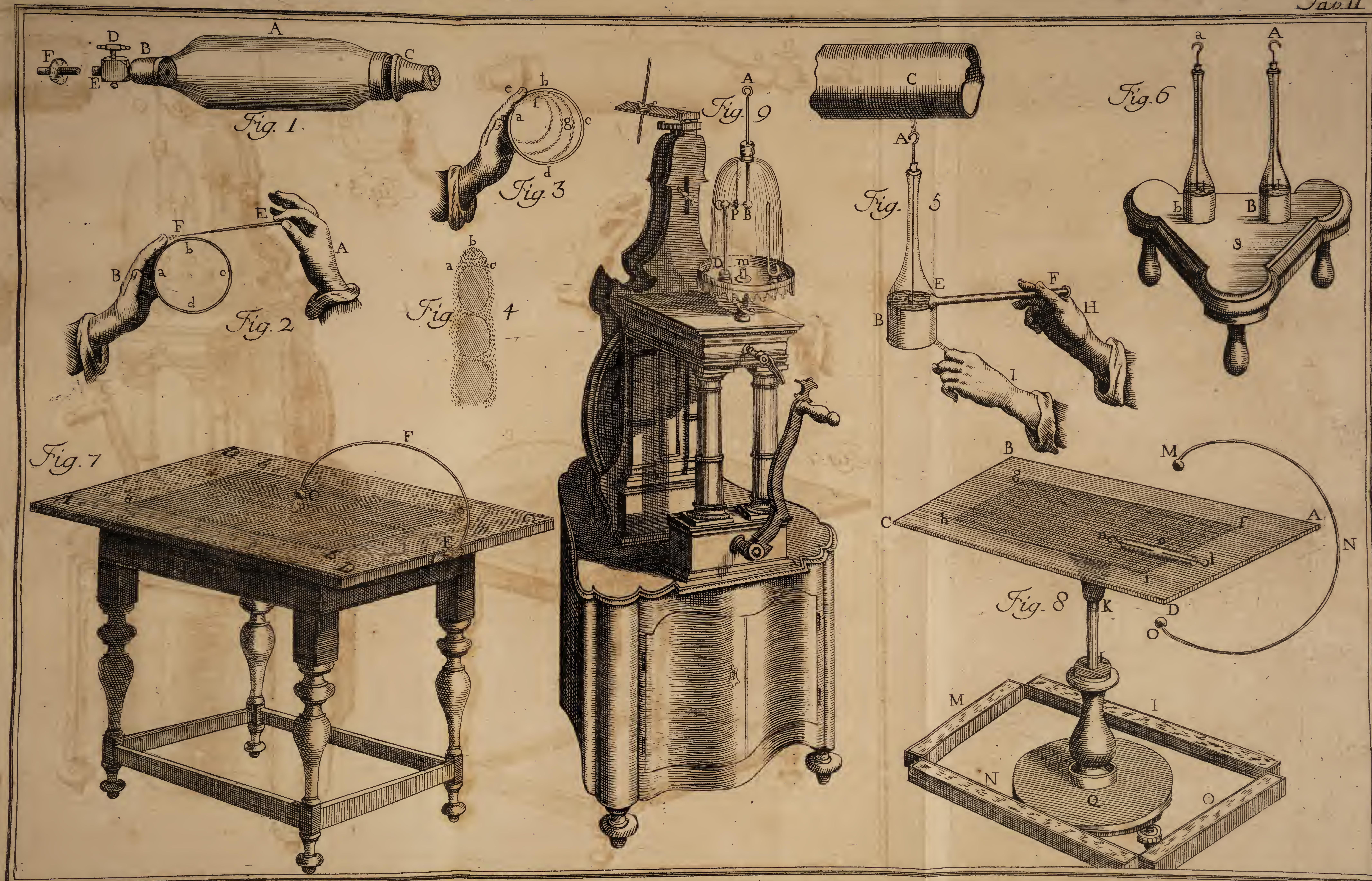
On the law of indication.

112. IN general, this law of indication confifts in the difference of the appearances exhibited by the electric fire, when iffuing from a blunted point of metal (Pl. I. Fig. 2, 3.) placed in fuch fituations that the electrical fire may, according to the theory, iffue from one of the fyftems and pafs into the other. If the point has been annexed to that of the fyftems from which the fire *iffues*, and is properly directed towards a plain portion of the furface of the other, then the fire affumes an appearance that I have diftinguifhed by the appellation of a *brufh*; but when the point is annexed to the fyftem into which the fire *enters*, then the fire affumes another appearance, to which I have given the name of *little ftar*.

113. The *brufh* has the appearance of a little conical *fafcis* of rays, two thirds of an inch, or an inch; or even more, in length. The blunted part of that lucid cone correfponds to the blunted point of the metallic rod, and there the electrical fire has its greateft denfity: from fuch point, the fire fubdivides itfelf into rays which grow continually more numerous, and proportionably thinner, as alfo more languid. Neither are thofe rays perfectly continuous; but, if you obferve attentively, you will fee that they vibrate in an interrupted manner; and you may obferve likewife a noife, or rather feries of little cracks, feparated by little intervals of time, which exactly correfpond to the above-mentioned interrupted vibrations; thefe cracks arife, as do all other founds, from the motion of the air, and fhew by their interruption that the electrical fire ftrikes the air with a correfponding interruption. This interruption is greater as the metallic rod is blunter; fo that if the point of it be extremely obtufe and placed very near the fyftem into which the fire paffes, the brufh then degenerates to a feries of little fparks evidently feparated from each other. On the contrary, as the point is fharper, the rays that form the brufh grow lefs divergent, fhorter, and more continuous; and the noife degenerates to a whiftling, or noife more acute and continuous.

114. The

Tab. II.



114. The *little* electric *ſtar*, as we call it, is very eaſily diſcerned from the *bruſh:* the light of it extends itſelf to ſuch a ſhort diſtance that it can hardly be ſaid to form the beginning of a cone, ſo that many perſons give it the appellation of the *lucid point;* this little ſtar produces likewiſe a little whiſtling, but all other circumſtances being equal, leſs than that produced by the bruſh; and in the ſame manner as the vibration and interruption of the rays of the bruſh is leſs manifeſt at the top of it, where its fire is more denſe and affords a more vivid light, ſo in the ſhort and pretty vivid light of the little ſtar, ſcarcely any interruption is to be diſtinguiſhed.

115. I ſhall treat of the other accidents of thoſe lights in a chapter apart, where I propoſe beſides to enquire into the reaſon of their formation and effects; our preſent object being more particularly to define the laws which the poſition of thoſe lights follow. I. In none of the ſyſtems, either a bruſh, or a little ſtar, ever appears on a point ſo annexed to it as to turn back again toward the ſame ſyſtem. II. There is always either a bruſh, or a little ſtar, on a point annexed to the one of the ſyſtems, and directed to the other. III. If the point annexed to the one ſyſtem, exhibits a bruſh directed towards the other, the latter then exhibits a little ſtar, correſponding to the former. IV. Of the three ſyſtems, the two which are animated by the friction of the glaſs, always exhibit the ſame kind of light, that is, the Chain always a bruſh, and the Machine always a little ſtar; which lights are more vivid in both, when they take place between one another, than when they ariſe between them and a ſtrange body: but the third ſyſtem, that is, the ſtrange bodies, which is not immediately animated by the friction of the glaſs, exhibits either the bruſh or the little ſtar, viz. always the little ſtar with reſpect to the Chain, and always the bruſh with reſpect to the Machine. V. The friction of the glaſs being once ſuppoſed, the vividity and duration of the bruſh from the Chain, relatively to ſtrange bodies, or of the little ſtar on ſtrange bodies, relatively to the Chain, is proportioned to the ſmall, or great, or infinite capacity of the Machine; and *vice verſâ*, the vividity and duration of the bruſh on ſtrange bodies, relatively to the Machine, and of the little ſtar on the Machine

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relatively to ſtrange bodies, is proportioned to the capacity of the Chain, whether it be ſmall, or great, or infinite. VIthly, and more univerſally: the bruſh and little ſtar are ſubject to the ſame laws of *unity*, *exiſtence*, *non-exiſtence*, *connection*, and *proportional diſtribution*, as the other electric ſigns.

116. Now, we therefore muſt lay it down as an acknowledged truth, that the *bruſh* is formed by the electrical fire that iſſues, and the *little ſtar* by the ſame fire, when it enters, ſince both the ſaid bruſh and little ſtar, are ſubject to all the ſame laws to which the other ſigns are ſubject (115, VI. of this the Franklinian theory is an immediate conſequence) that is to ſay, I. All electrical ſigns are formed by the ſubſtance of the electrical fire when it iſſues from one ſyſtem to enter the other. (115. III.) II. The electrical ſigns that take place between the Chain and ſtrange bodies, are produced by the electric fire which iſſues from the ſame Chain, and diffuſes itſelf into the ground: the ſigns between the ground and the Machine are produced by the fire that iſſues from the ground and enters into the Machine; and the ſigns between the Chain and the Machine, are formed by the fire, which, with a ſtill greater force, iſſues from the Chain and enters into the Machine. (115. IV.) III. The fire that ruſhes from the Chain is proportioned to the quantity which the capacity of the Machine can ſupply; and the fire which from the ground may enter the Machine, is proportioned to the quantity which the glaſs can tranſport from it into the Chain. (115. V.) IV. Hence the Chain is electric by exceſs with regard to ſtrange bodies; the Machine is electric by deficiency with reſpect to the ſame bodies; and the Machine and the Chain are electric per exceſs and deficiency, with reſpect to each other.

117. Conſequently, the contrary reciprocal effects of the bruſh and little ſtar, the diverſity of their viſible form, and the identity of the ſubſtance from which they proceed, all theſe claim this hypotheſis, viz. that the bruſh is an electrical fire that iſſues, and the little ſtar an electrical fire that enters. The contrariety of their reciprocal effects is manifeſt from this; if any inſulated body whatſoever, be placed in the middle between the ſaid contrary ſigns when theſe are ſimilarly effected, or between ſimilar, but

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contrarily difpofed figns, the one deftroys the electricity which the other communicates. I infulate myfelf and prefent my right hand to a point annexed to the Chain, and the left to a point annexed to ftrange bodies; on the one the brufh appears, on the other the little ftar, and the refult is that the one fuppreffes the electricity raifed by the other. The fame obtains, if being likewife infulated, I prefent with my right hand a point to the Chain, and with the left I prefent another to ftrange bodies; the brufh from the latter will deftroy the electricity introduced by the former. The effect will again be the fame, if I prefent one of my hands to a point annexed to the Chain, and a point to ftrange bodies, a brufh from the latter will deftroy the electricity introduced by the brufh from the former; laftly, the fame will obtain ftill, if I prefent a point to the Chain, and my hand to a point annexed to ftrange bodies. Therefore, thofe figns which, according to the hypothefis, ufually manifeft fuch fire as actually proceeds from the infulated body to ftrange bodies, deftroy the electricity introduced by thofe figns which are ufed to manifeft fuch fire as actually proceeds from the Chain to the infulated body.

118. Now, if the contrariety in the reciprocal efficiency of the brufh and ftar upon each other, does not proceed from contrary motions taking place in the electrical fire that forms both, we muft neceffarily have recourfe, in order to explain the aforefaid contrariety, to the combination of two different fubftances, which, when united, reciprocally deftroy that electricity which, when feparated they are able to raife; but the poffibility of fuch a diverfity of fubftances is excluded by the fimiliarity of all the perceptible qualities, of the fubftance that produces the brufh, and of the fubftance which produces the ftar, and the identity of all the effects produced by both.

119. Indeed, as far as our fenfes can penetrate into the faid brufh and ftar, it evidently appears that both are formed of a fubftance which fhows itfelf to be equally fluid, equally capable of producing light, equally free of any fumes, exhalation, and makes on all our organs the moft identical impreffions, except indeed where fome difference arifes from a difference in the quantity of it; but then it by no means follows, that there is a diverfity in the fub-

ſtance itſelf, ſince it is even poſſible at all times to transform the ſtar into a ſpurious bruſh, and the bruſh into a ſpurious ſtar. To effect ſuch a transformation, I uſe as points, ſmall rods of braſs, one eighth of an inch thick, and only rounded at their extremities; when from the one of theſe a moſt vivid bruſh ſprings out, I preſent another point in an oblique direction, and it happens that at a certain degree of obliquity and at a certain diſtance, the rays of the bruſh, on the former point, conſiderably loſe of their divergency, and turn as if they intended to unite together on the point of the latter; but afterwards they diſappear, and are rendered again viſible at a certain diſtance from the ſaid rod, where they again unite together under the ſhape of a bruſh.

120. The transformation of the bruſh is more obvious; we ſhall ſee in its proper place, how it is ſufficient for making the fire exhibit the appearance of a ſtar, to make it ſpring from a very ſharp point, or to a blunted point to preſent a ſharp one.

121. And theſe transformations do in no wiſe deſtroy the ſignification of the two ſigns. For concluding that the electrical fire actually iſſues from a given ſyſtem, or enters into it, it is not material whether it always iſſues or enters with a ſame appearance, and ſuffers no change in that reſpect from any combination that may have been made between the form and poſition of the body from which it iſſues, and the form and poſition of that into which it enters, and from any ſubverſion, as it were, of the ſaid *data* and combination of other circumſtances; it is ſufficient that the given ſyſtem at the ſame time that it throws a bruſh from a point annexed to it, makes at the ſame time a little ſtar appear on a point annexed to the other ſyſtem.

122. Meanwhile, a diligent obſervator will not fail to obſerve, eſpecially in the transformation of the ſtar, certain accidents which, though they may of themſelves exhibit the appearance of a ſpurious bruſh, (119.) yet merely proceed from the rays of the true bruſh. Becauſe, I. At a certain diſtance, and with a certain obliquity of the two rods, one part alone of the rays of the true bruſh, that is, the neareſt rays, will deviate and proceed pretty directly towards the other rod, in order there to form the bruſh; while the fartheſt will gradually deviate leſs. II. And the rays of

of the ſpurious bruſh will correſpond one by one, to thoſe rays alone of the true bruſh, which will deviate enough to proceed from the rod annexed to the Chain, to that which from the ground is preſented obliquely to it. III. And, as the rod that communicates with the ground is brought gradually nearer, other and other rays, which at firſt deviated leſs towards it, will gradually deviate more. IV. Such ſucceſſive deviation of the true bruſh will ſtill more evidently appear, if inſtead of the rod, I preſent obliquely the extremity of my finger; in ſuch caſe the ſpurious bruſh ſhortens ſtill more, and draws ſtill nearer to the appearance of a ſtar. V. But a ſtill better exhibition of the motion of the true bruſh in the caſe of the N° 115. may be given. Two inſulated men communicate, the one with the Chain, the other with the Machine; the former preſents the rod, partly to the finger of the latter, partly to the finger of another man who ſtands on the ground; and then the bruſh that ſprings from the rod held by the man communicating with the Chain, exhibits a ſpectacle equally agreeable and ſignificative; this bruſh approaches itſelf with a kind of preference to the man who communicates with the Machine, its deviation towards him begins at a greater diſtance than uſual, is more conſiderable, and takes place ſooner; the diverging rays are more re-united and thrown at a greater diſtance. All which lays, as it were, the theory under our eyes, or at leaſt is moſt conform to it, and extremely capable to enable us to diſtinguiſh the true bruſh from the ſpurious, and to preſerve to the former its ſignification. Mean while, it is eaſy to perceive in the pretty long rays of the ſpurious bruſh, the identity of the ſubſtance deſtined to form the ſtar, with that which forms the bruſh: which is the object we propoſed at firſt to demonſtrate.

123. But ſuch identity is ſtill better confirmed by the identity of the effects produced by the ſubſtance which forms the bruſh, and that which forms the ſtar. Let us ſuppoſe for a moment that thoſe two ſubſtances are different: now, ſince all other vivid electric ſigns are ſubject to the ſame laws as the ſaid bruſh and ſtar, it follows from ſuch ſuppoſition the ſubſtances that concur in the formation, for inſtance, of a ſpark, will be alſo different; and when, with the knuckle of my finger, I excite a ſpark from the finger of

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the man A, the fubftance *formatrix* of the brufh, and that which produces the ftar, muft therefore both concur in the formation of the fpark raifed between the man A and me, fince fuch fpark is the refult of the force that determines thofe different fubftances to move towards each other, in order to meet and unite. Therefore, in any point between the fingers at which thofe fubftances may be fuppofed to meet, a reafon will fail why they fhould proceed farther; whence it muft follow that the finger of the man A will be pricked for the greateft part by the fubftance that forms the brufh, and my finger by that which forms the ftar: but how can fuch moft fimilar punctions be produced by fubftances of a different nature?

124. Moreover, let the extreme terms of the fpark be changed, let its intenfity be increafed, let even the medium through which it paffes be changed; that is, let an efficacious fpark be drawn through any fuch body as ufually refufes a paffage to it, either in confequence of its own infulating nature, or of its fmall capacity, then the fpark will fhiver it to pieces, or reduce it into fmoke, or calcine or vitrify it, and in a word, alter it conformably to its peculiar nature, though alter it in the moft identical manner, even in thofe parts which are only applied to each other, and where the fubftance of the brufh alone might then be fuppofed to operate on the one of thofe parts, and the fubftance of the ftar, on the other.

125. But in order to manifeft ftill more the feeblenefs, and even the contradiction of the theory of the two diftinct fubftances, I think that I muft above all produce the moft elaborate and ingenious expofition that has been given of it till this day; fuch is that which Dr. Prieftley has endeavoured to give, and has fet in its moft advantageous light in his valuable Hiftory of Electricity, which he publifhed in London in the year 1767, and which he has enriched with many new experiments and obfervations of his own.

126. "Let us fuppofe (fays he) that there are two electric fluids, "which have a ftrong chymical affinity with each other, at the "fame time that the particles of each are as ftrongly repulfive of "one another. Let us fuppofe thefe two fluids, in fome meafure, "equally attracted by all bodies, and exifting in intimate union "in their pores, and while they continue in this union to exhibit "no

" no mark of their exiftence. Let us fuppofe that the friction " of any electric produces a feparation of thefe two fluids, caufing " (in the ufual method of electrifying) the vitreous electricity of " the rubber to be conveyed to the conductor, and the refinous " electricity of the conductor to be conveyed to the rubber. The " rubber will then have a double fhare of the refinous electricity, " and the conductor a double fhare of the vitreous; fo that, upon " this hypothefis, no fubftance whatever can have a greater or " lefs quantity of electric fluid at different times; the quality of " it only can be changed.

127. " The two electric fluids, being thus feparated, will begin " to fhow their refpective powers, and their eagernefs to rufh " into reunion with one another. With whichfoever of thefe " fluids a number of bodies are charged, they will repel one an- " other, they will be attracted by all bodies which have a lefs " fhare of that particular fluid with which they are loaded, but " will be much more ftrongly attracted by bodies which are " wholly deftitute of it, and loaded with the other. In this cafe " they will rufh together with great violence.

" 128. Upon this theory, every electric fpark confifts of both " the fluids rufhing contrary ways, and making a double current. " When, for inftance, I prefent my finger to a conductor loaded " with vitreous electricity, I difcharge it of part of the vitreous, " and return as much of the refinous, which is fupplied to my " body from the earth. Thus both the bodies are unelectrified; " the balance of the two powers being perfectly reftored.

" 129. When I prefent the Leyden phial to be charged, and, " confequently, connect the coating of one of its fides with the " rubber, and that of the other with the conductor, the vitreous " electricity of that fide which is connected with the conductor " is tranfmitted to that which is connected with the rubber, which " returns an equal quantity of its refinous electricity; fo that all " the vitreous electricity is conveyed to one of the fides, and all " the refinous to the other. Thefe two fluids, being thus fepa- " rated, attract one another very ftrongly through the thin fub- " ftance of the intervening glafs, and rufh together with great " violence, whenever an opportunity is prefented, by means of

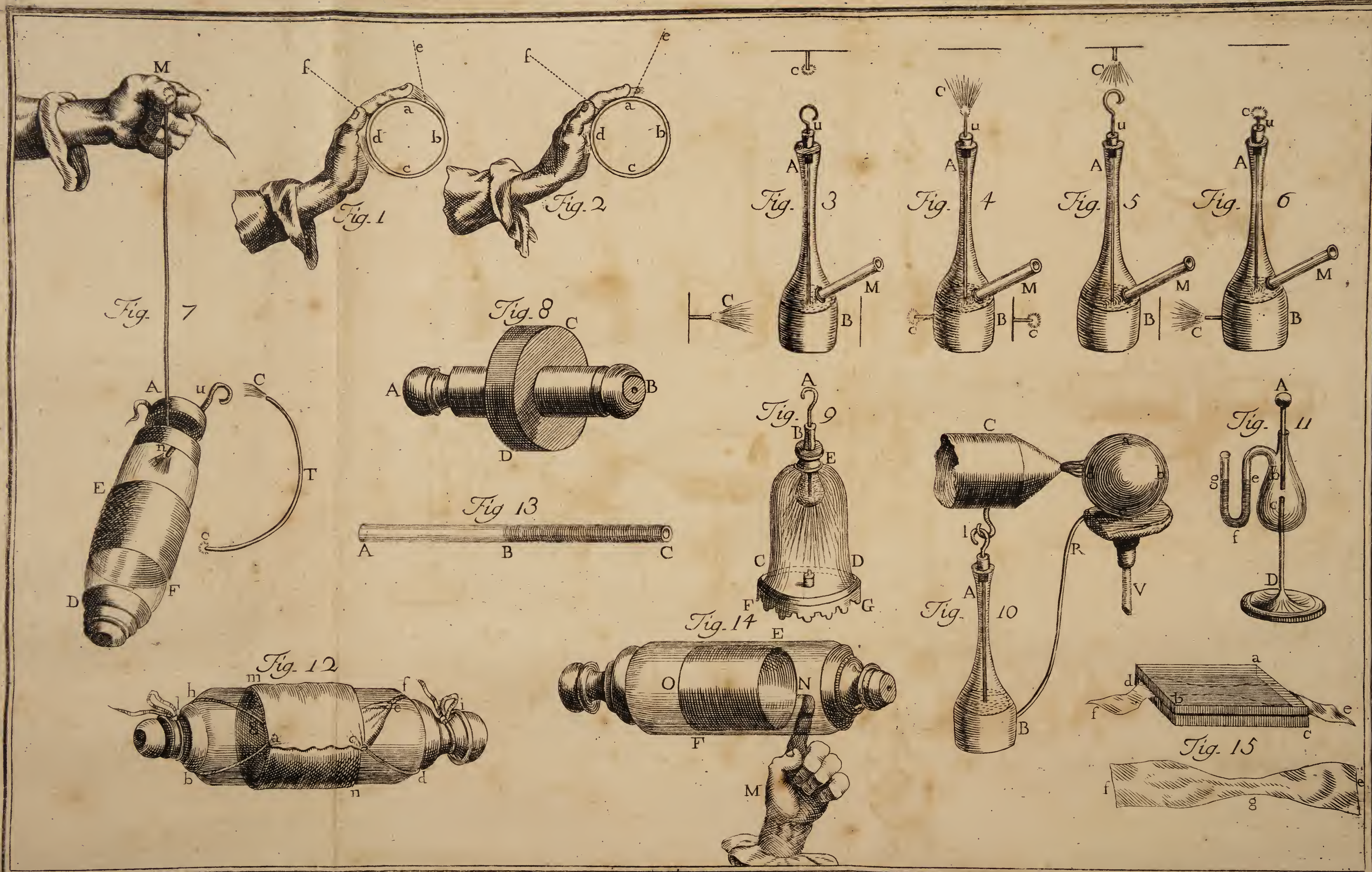
" proper conductors. Sometimes they will force a paſſage through
" the ſubſtance of the glaſs itſelf; and, in the mean time, their
" mutual attraction is ſtronger than any force that can be applied
" to draw away either of the fluids ſeparately."

Such are the principles on which Dr. Prieſtley eſtabliſhes the hypotheſis of the two fluids; and I think I muſt here obſerve that the exiſtence of two fluids being once ſuppoſed as fact, the principles above appear to me neceſſary, or at leaſt very proper to explain a few particular facts, or at leaſt a few which can admit more eaſily of ſuch explanation, and Dr. Prieſtley, it muſt be confeſſed, has ſet forth and ſupported the ſaid hypotheſis as ingeniouſly as well could be; but on the other hand I muſt add (did I omit it, it would be doing injury to the ſaid ingenious author) that in his poſterior experiments and obſervations, he has uſually followed the Franklinian theory.

130. After thoſe previous obſervations, I ſhall make ſome reflections on the abovementioned principles, and ſhall diſſemble none of the conſiderations with which Dr. Prieſtley has endeavoured to confirm them. And firſt, with regard to the chymical affinity of two fluids, it is true, as Dr. Prieſtley alledges in line 27. page 470. that nature abounds with inſtances of affinities of this kind, which, when the ſubſtances are united, do not manifeſt their peculiar force, though this force is exerted in ſuch a remarkable manner when theſe ſubſtances happen to be ſeparated: but indeed I have no knowledge that there can be in nature any inſtance of two diſtinct, nay contrary ſubſtances, ſuch as muſt be the vitreous and the reſinous fluid, which, when ſeparated, may have all their outward manifeſtations exactly alike, and produce the very ſame effects, the one as the other; beſides that ſuch an affinity, however convenient it may appear for the explanation of the natural equilibrium of the two fluids, is entirely repugnant to the inequilibrium, or *unbalancement* of one.

131. Beſides, how is it poſſible for the friction ever to ſurmount the union reſulting from ſuch a chymic affinity? The friction uniformly excites the contrary kinds of electricity in the ſeveral parts however diſtant, of the Machine or Chain, which in the Franklinian theory naturally and neceſſarily follows from the mere expanſive force of one ſame fluid. It is enough that the friction ſhould

Tab. III



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increaſe in any manner whatever, the expanſive force of that portion of electric fluid which is on the ſurface of the hand, or of the rubbing cuſhion, or that the ſame friction ſhould diminiſh the expanſive force of that portion of electric fluid which is on the ſurface of the glaſs, for producing a tranſlation of a portion of that fluid from the hand to the glaſs; and the ſame circumſtances likewiſe ſuffice for making the fluid in any part, however remote, of the Machine, to diſtend itſelf proportionably to the quantity wanting in the place of the friction, and cauſing the *exceſſive* fire carried by the revolving glaſs to the Chain, to paſs into the ſame, and create in all parts of it an *exceſſive* denſity. But in the theory of two fluids, how the action, entirely mechanical, of the friction (which does not of itſelf extend farther than thoſe parts which are actually brought into a contact with each other) can it poſſibly ſeparate in very diſtant places and by a poſitive force, different parts of two fluids which are cloſely united with one another? A particular principle, or force, ſhould at leaſt exiſt, which, after the ſeparation of thoſe parts of the two fluids which are near the two rubbing ſurfaces has been effected, could determine the other parts diffuſed throughout the whole extent of the Chain or the Machine, to paſs to one ſame place, in order there to be in their turn diſunited and ſeparated. Now, the hypotheſis of two fluids does not mention any ſuch active principle; on the contrary, it ſuppoſes a force that keeps all the parts of the fluids united, and the excogitator of it moreover confeſſes (p. 475. l. 11.) that each of the fluids is attracted by all bodies around it, with a force at leaſt equal to that with which they reciprocally attract each other.

132. I here omit to mention that the electricity in the Machine, and the Chain, are animated in a very different way: in the Machine it is animated by a friction of a part of the ſame againſt the glaſs; in the Chain, by a mere paſſive vicinity of the ſame to the glaſs. This, in the Franklinian hypotheſis, is entirely conſiſtent: it ſuffices that the glaſs, through the mechanical force of the friction, takes a quantity of electric fire from the Machine, to make the ſame diffuſe itſelf, in conſequence of its natural expanſive force, into the Chain. But in the hypotheſis of two fluids, it becomes neceſſary that the

the friction, which only obtains between the glafs and a very fmall part of the Machine, fhould equally feparate the refinous from the vitreous fluid, both in the Chain and the Machine; and the former muft as well be tranfported from the Chain into the Machine, as the latter from the Machine into the Chain.

133. But Dr. Prieftley is indeed far from being perfuaded that the friction (which, we muft obferve, is acknowledged in both hypothefes to be the only caufe of the contrary electricities in the Machine and the Chain) can without a contradiction feparate the two fluids, and reciprocally tranfport the refinous fluid of the Chain into the Machine, and the vitreous fluid of the Machine into the Chain: on the contrary, he really feems to have perceived that things were otherwife, and not to have found any other better way of favouring his hypothefis, but by fubftituting the unfupported words *let us fuppofe*, to argument and facts.

134. The disjoining of the two fluids being once gratuitoufly fuppofed, the fame gentleman paffes to the application of his hypothefis to electrical motions; to which motions the faid hypothefis appears to him extremely conform, while he thinks the Franklinian theory infufficient: *indeed Dr. Franklin himfelf*, fays Dr. Prieftley, in p. 472. *ingenuoufly acknowledges that he was a long time puzzled to account for bodies that were negatively electrified repelling one another.* The fame difficulty likewife occurred to me at firft; but it had not with me, and I think with great reafon, the fame weight it muft have had with others, becaufe, I. Even then I did not confider the divergences as being produced either by the excefs, or the defect of the electric fire, abfolutely confidered; but indeed from the inequality between the fire in the bodies, and that in the ambient air (Chap. II.); and as this inequality fubfifts in the cafes of bodies electric by excefs, as well as in the cafes of bodies electric by deficiency, the divergence in both cafes appeared to me equally natural. II. Afterwards, in the year 1760, the confideration of the electricity of the vaporous air (the experiments relative to which I had expofed in the feventh letter on the terreftrial atmofpheric electricity) induced me to write a letter to Dr. Franklin, publifhed in the fecond part of the 50th volume of the Philofophical Tranfactions,

in which I explained more precifely the divergence of bodies equally electric, by the fole preffion, either of the fire fuperabounding in bodies againft the fire in the air; or the preffion of the fire in the air, againft the fire in fuch bodies as were deficient. *Si ignis proprius corporum, quantufcumque is fit* (I fay in p. 522 of the faid volume) *libratur cum igne ambientis aëris, corpora . . confiftunt in nativo ftatu; . . . fin alter ignis fuperat alterum, corpora difcedunt, vi ignis fuperantis.* 3dly, and laftly, Since the time I fucceeded in analyfing the phenomena of the electric *well*, and thus to afcertain the nature of electric atmofpheres, I have continually found the explanation above more true and confentaneous to all manner of facts. I here defire the reader to confult my little book *de Atmofphærâ Electricâ, ad Regiam Londinenfem Societ. Taur.* 26 *Feb.* 1769. The fire inherent to the air tends to balance itfelf, but without mixing with the moveable fire in the deferent bodies; and in the fame manner as a fame kind of motion is perceived in the balance, whether I put a new weight in the fide A, or take an equal weight from the fide B, thus two bodies equally electric always will equally diverge, whether the exceffive fire of the bodies furpaffes the natural fire in the air; or the natural fire in the air, furpaffes the deficient fire in the bodies.

135. But we are to difcufs all this in time, with more precifion and accuracy; now it fuffices, after having fhewn that divergences are no wife repugnant to the Franklinian theory of a fingle fluid, to obferve, that though thefe divergences may at firft fight feem to favour more the theory of the two fluids, yet, in examining more clofely into the fubject they are found to render that theory much lefs probable. I omit, for the prefent, the difficulty of feparating the two fluids mentioned above; I only confider here the impoffibility of conciliating the identity of the effect produced by each of thofe fuppofed fluids feverally, in the body in which it is fuppofed to predominate, with the fuppofed difference of their nature, in confequence of which, it is faid, they mutually attract themfelves. This is the character of a philofophy illufive, and too lazy to inquire into the caufes of things, thus to imagine fluids that muft have fuch motions as cannot take place in bodies. Bodies differently electrified mutually attract each other: Why?

 Becaufe

Becaufe they are animated by two fluids that mutually attract each other. Bodies fimiliarly electrified mutually repel each other: Why? Becaufe they are animated by two fluids, the particles of each of which repel each other, and that in a moft fimilar manner. But that thefe are by no means the principles and ways of nature, is manifeft from the exact unity of the effects of thofe fuppofed fluids, which unity is abfolutely repugnant to the fuppofed diverfity of their natures; from the impoffibility of their feparation, on which feparation, neverthelefs, every one of their manifeftations muft depend; laftly, from the manner after which they fhould unite again; which would be intirely oppofed to all phenomena.

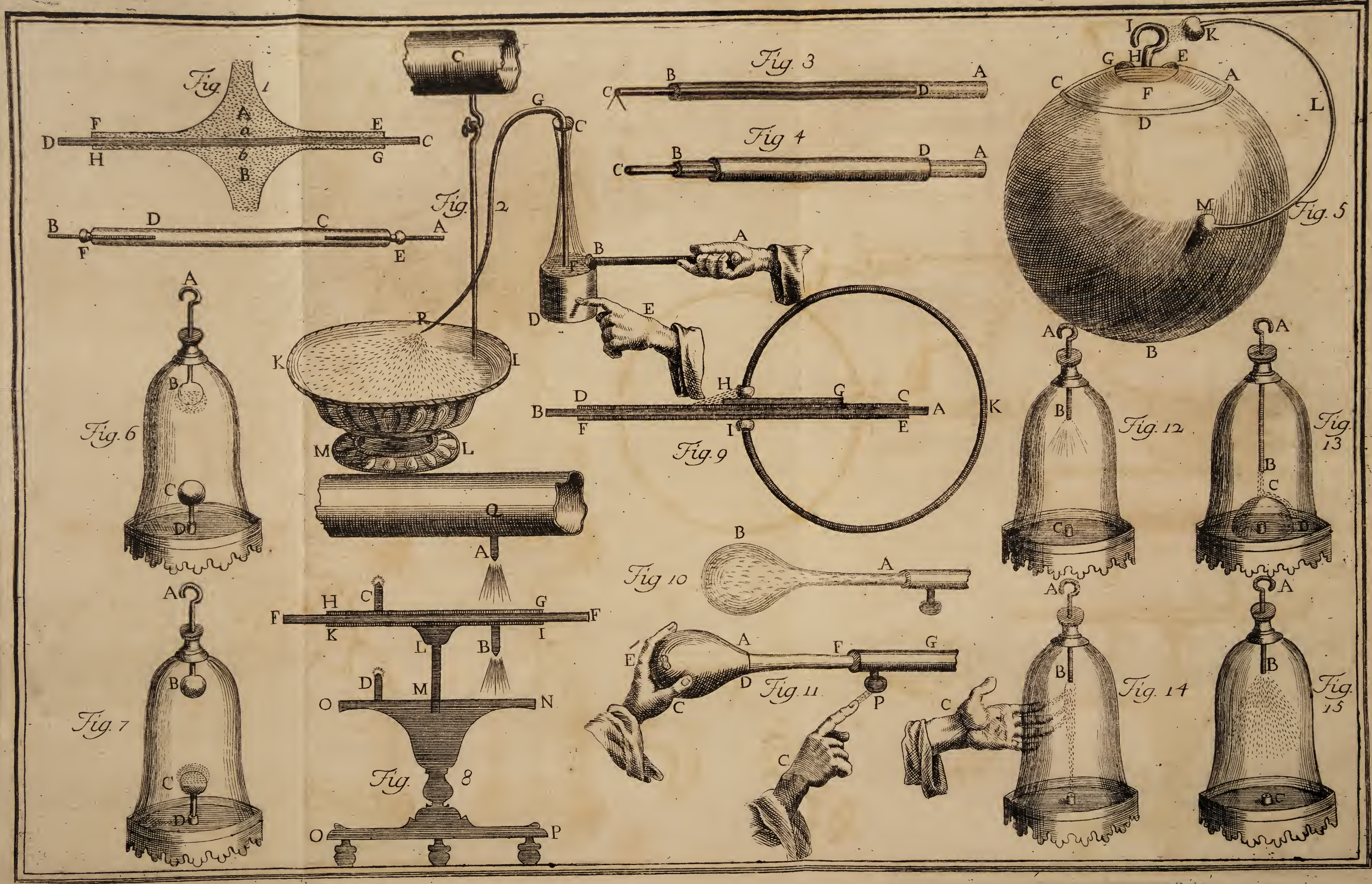
136. With regard to the two laft circumftances, "If it be afked, "(to ufe the words of Dr. Prieftley, p. 475.) why the two fluids, "meeting on the furface of the globe, or in the electric explo-"fion, do not unite, by means of their ftrong affinity, and make "no further progrefs; it may be anfwered, that the attraction "between all other bodies and the particles of both thefe fluids "may be fuppofed to be, at leaft, as ftrong as the affinity between "the fluids themfelves; fo that if any body is difpoffeffed of one, "it may recruit itfelf, to its ufual point of faturation, from the "other *." But, indeed, to attempt, in an hypothefis contrived as this is, to refolve one difficulty, only ferves to raife a number of others. Firft, if the fluids are more ftrongly attracted by the bodies in which they are contained, than they attract each other, certainly the difficulty of feparating them muft become double; fince we muft, befides their affinity and mutual attraction, alfo furmount this new common attraction that keeps them united to the faid bodies: to this add, that fuch attraction muft be furmounted only with regard to the fluid that is to be feparated: befides, would not fuch an attraction from the bodies oppofe the repulfive force of the feparated fluids? Secondly †, how could thofe impulfions of any one of the

* Let us obferve, that in the furface of the glafs, at leaft in that portion of it that proceeds from the hand towards the Chain, the electricity by excefs actually predominates; which would be impoffible in the hypothefis of the two fubftances.

† I fuppofe ftill that thefe words *let us fuppofe* have the miraculous efficacy to caufe the two fluids, viz. the refinous from all fides of a Chain ever fo extenfive, and the vitreous from all fides of a Machine, let its capacity be ever fo great, to run to the glafs.

fluids

Tav. IV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fluids towards the Machine and the Chain, inftead of balancing each other, furmount the mutual attractions of the fluids towards each other, and their common attraction towards the glafs; and operate fo as to make thofe fluids continue to exchange their ftations and proceed, viz. the vitreous into the Chain, and the refinous into the Machine. The fame reafoning muft be applied to the cafe of two fluids which are fuppofed to encounter each other in the line in which an explofion is effected: there is no doubt but fuch explofion can be effected through a deferent body, for inftance, through an iron wire feveral miles long. Now, why fhould thofe fluids, which are fuppofed to meet in the middle of the faid line, inftead of being ftopped there by the efficiency to their affinity, and of the attraction of the iron within which they are fuppofed to run, why fhould they, I fay, preferve their motion and proceed farther? This furely muft be in confequence of fome attraction from the furface of the glafs; but then, it is requifite that fuch attraction fhould be infinite (fince it muft be fubject, like all kinds of attractions in nature, to fome inverfe law of diftances) with regard to the fum of the abovementioned forces.

137. I omit, as being abfolutely fuperfluous, to mention the other arguments by the help of which Dr. Prieftley continues, in page 475, to endeavour to procure fome probability to the hypothefis of two fluids; which indeed of itfelf can have none: and whoever will confider it with any degree of attention will be made ftill more fenfible of its contradictions than I have been able here to manifeft. The able hiftorian of electricity has himfelf given the beft confutation of his own theory of two electric fubftances, the refinous and the vitreous, by attempting to apply it to general facts, though, it muft be confeffed, he has in the mean time ufed all poffible induftry to reconcile his theory to thofe facts.

138. I think I cannot better confirm the theory of a fingle fluid, than by producing here a moft fignificative experiment, which I fent the 14th of January, 1766, to the Royal Society, and which is inferted in the volume of the fame year. Let two balls of brafs exactly polifhed B C, two thirds of an inch, or an inch in diameter, be placed under a bell of glafs (Pl. IV. fig. 6, 7.) at

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the diſtance of about five inches from each other. The ball C riſes from the baſon of the air-pump; the other B is annexed to the rod A B which communicates, through the neck of the bell, with the Chain. I place the bell upon the air-pump (Pl. II. Fig. 9.) and, when the air is carefully extracted, I rub the glaſs ſoftly, in order to excite in the Chain only a very weak electricity; meanwhile I obſerve, and explore the preciſe degree to which the electrical fire manifeſts itſelf on the ball B, without paſſing, at leaſt viſibly, to the ball C: uſually I obtain ſuch preciſe degree, by applying the palm of my hand to the ſide of the bell, taking care not to touch the braſs with which the neck is garniſhed: the conſequence of which is, that as a great part of the bell is externally *coated*, a great portion of fire flows along that part of the interior ſurface which correſponds to my hand, and it at laſt happens that that preciſe portion becomes retarded, which is neceſſary for producing the deſired appearance.

139. This appearance conſiſts of a moſt agreeable and regularly variegated hemiſphere of light, which riſes from the loweſt hemiſphere of the ball B in *b*, and covers the ſame, while, at the ſame time, no other light is perceived, either on the other ball C which communicates with the ground, or in the ſpace between both. The diſpoſition and degradation of that hemiſphere conſiſts of a ſeries of circles having for centre the loweſt point of the ball, and growing gradually larger as they riſe around the inferior hemiſphere of it. I. The light which produces the rings formed between the ſucceſſive circles, is equally rare, and equally languid in all. II. That height, beſides, to which the light reaches, is greateſt in the common centre of all the rings, and from thence goes regularly decreaſing, according to the greater diſtance at which the ſucceſſive rings ſtand from that centre. III. The rarity, and languidity of the light, increaſe after a ſame manner.

140. Now the manner and order, after which that light, which is of one and the ſame kind, riſes from the ball that communicates with the Chain, does certainly ſuffice to render viſible to the eye, I think I may ſo expreſs myſelf, the theory of a ſingle fluid, and to confirm all that has been ſaid before, to prove that the bruſh is produced by ſuch a ſingle fluid iſſuing from a ſyſtem that is in a ſuper-

ſuperabounding ſtate relatively to another. But the preceding experiment will receive a new degree of ſignificancy from the following one, which is ſimilar to it. I make the end of the rod A B which communicated before with the Chain, communicate now with the Machine; and the ſame agreeable and regularly variegated hemiſphere of light now covers the upper hemiſphere of the ball C, which has been made to communicate with the baſon of the air-pump, that is, with the ground: in the mean time, no light appears any where elſe, neither around the other ball B, which now communicates with the Machine, nor in the ſpace between the two balls. If I intend now to have the light appear around the lower ſurface of the ball B, the place where it appeared when the rod A B (Pl. IV. Fig. 6.) communicated with the ground, I inſulate the air-pump and make the baſon of it communicate with the Machine; I then make the end A of the rod A B communicate with the ground; and then the light appears again in B.

141. In ſhort, thoſe experiments evidently prove that the electrical matter manifeſts itſelf on that ball only in which the electrical fire finds itſelf in a ſtate of a relative exceſs: that is, on that ball which communicates with the Chain, and in which the fire is exceſſive, while, in the mean time, the other ball communicates with the ground in which the fire is in the natural ſtate; and on the other hand, the fire manifeſts itſelf on the ball that communicates with the ground, when the other ball happens to communicate with the Machine, in which the fire is deficient with regard to the ground: in ſhort, the light appears on the ball annexed to that ſyſtem which would, through a point, throw the bruſh toward the other. Therefore it is manifeſt, that ſuch a light (could not we call it the light of *diffuſion*) whether we conſider its diſpoſition and form, or examine either its ſituation or unity, allows no poſſibility to the theory of two diſtinct electrical fluids.

142. But the comprehenſive ſignification of this light of *diffuſion* is found, on the other hand, to increaſe beyond meaſure, when we attend to another light that I call, and which is in fact, the light of *overflow*. This light appears likewiſe on one of two balls B C placed near one another, under a bell emptied of air: it is a

 matter

matter of indifference whether they be placed either the one under the other, or both in an horizontal plane. The vacuum being formed, and the hook of the rod being joined with the Chain, we may then obferve a cylinder of a weak reddifh colour, flowing from the ball B which communicates with the Chain, toward the ball C which communicates with the ground, and forming there a fmall light of *overflow*, fuch as is delineated in Pl. V. Fig. 10. When the electricity is weak and the ball C exactly communicates with the ground, this light becomes like a moft fubtle fur, or veil, which fpreads itfelf around the bafis of the fmall cylinder of light, in the place where this reaches the ball C, and even fuch light does not appear but very imperfectly, when the place in which the experiment is made is not perfectly dark.

143. But then that light of overflow will become completely vifible if a bottle be difcharged between the two balls; a little difcharge will produce a fmall light, fuch as it is reprefented in Pl. V. Fig. 10. A greater difcharge will prefent a light proportionably greater, as in Fig. 11. And a ftill more copious difcharge will make the light of *overflow* plentifully furround both the whole ball C, and a part of the rod on which it is raifed, and even fpread itfelf around the ball B (Fig. 12.)

144. I have hitherto fuppofed that the bottle is difcharged by directing its exceffive fire from the ball B to the ball C; now I direct it in a contrary way. In the laft experiment I held the bafon of the air-pump with one hand, and carried with the other the hook of the bottle, to the rod of the ball B; now, I carry the hook of the bottle to the bafon of the pump, and the refult of this change is, that the light of overflow then appears around the ball B.

145. This light, therefore, is of itfelf, a farther confutation of the theory of two fluids, and an ocular demonftration of the Franklinian theory of one fluid, which diffufes itfelf every where to an equality; for either that light is formed by one of the two fuppofed fluids, or by both at once, while they move from the one ball to the other: but if the light in queftion is formed by both, why does it rife on one ball alone, and not upon the other? If it

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is formed by one fluid only, why does it not riſe on that ball from which ſuch fluid moves?

146. But the extenſive ſignificancy of thoſe two lights, that is, that of *diffuſion*, and that of *overflow*, increaſes ſtill, if we compare their ſeveral accidents together, and examine the agreement between them and the theory of a ſingle fluid. The light of diffuſion manifeſts itſelf to a great diſtance, from one ball to the other; the light of overflow loves to keep to ſmall diſtance: the light of diffuſion appears, though neither the luminous cylinder or any other emiſſion of light takes place; the light of overflow does not appear but conſequently to the exiſtence of a cylinder of light; that is to ſay, in conſequence of an actual transfuſion of the electric fire: the light of diffuſion cannot receive any addition without degenerating to one continued ray; the light of overflow increaſes as the force of the diſploded fire increaſes. Laſtly, the former light appears on the ball from which the electric fire, according to the theory of one fluid, muſt iſſue; the latter, on the ball into which, according to the ſame theory, it is to enter: the former, on that part of the ball ſuperabounding with fire, which faces the deficient ball; the latter, on that part of the deficient ball which faces the ball that ſuperabounds with fire. The nature of the former light agrees extremely with the idea of a fire that iſſues with an inconſiderable degree of force, and which is not able to repel the reſidual air, however rare it may be and ſpread itſelf to any conſiderable diſtance; the latter, agrees entirely with the idea of a fire that enters, and which, ſtriking againſt the fire originally exiſting in the ball, recoils back againſt the moveable rare reſidual air: in ſhort, that appears in the ſame place that the bruſh does, this in the ſame place that the ſtar does. Therefore, it appears that the ſignification of the bruſh and the ſtar is fully demonſtrated, as well as the Franklinian theory, which reſults from the ſaid ſignification, or rather is contained in it: which was the principal object of this chapter.

CHAP. VI.

On the law of *excitation*.

147. HITHERTO, I have only ſpoken of the two animated ſyſtems, the Machine, and the Chain; and of the indifferent ſyſtem, or ſtrange bodies. The appearances exhibited by the bruſhes and the ſtars, excited between thoſe three ſyſtems are moſt diſtinct, and the other ſigns between them are alſo more deciſive, than thoſe on the *animating* ſyſtems. However, in order to throw a complete light on the ſubject, I ſhall now conſider the latter ſyſtem, that is, the glaſs; and diſcuſs that law which I call of *excitation*, in conſequence of which the electric fire paſſes from the rubbing hand into the rubbed glaſs: a part of this fire afterwards flows back into the hand, at that place where the glaſs begins to move from it; another portion diffuſes itſelf into the Chain, through that part to which the glaſs paſſes neareſt; and another portion gets back into the hand, at that part where the glaſs returns again to it.

148. Let us propoſe that law with more preciſion. *I. The electric fire, proper to the rubbing hand, paſſes inviſibly from thoſe points of it which are in contact with the glaſs to the ſurface of the glaſs, and adheres to it proportionably to the force of the friction, and to the quantity of fire exiſting in the hand itſelf. II. That fire which is thus collected upon and adheres to the glaſs in the time and in the place of the friction, from that inſtant begins to diffuſe itſelf from thoſe parts of it which, in conſequence of the rotation, begin to leave the contact of the hand; a part of the ſame fire thence flows back and gets again into thoſe points of the hand which lie neareſt to the glaſs, though no more in contact with it; while the remaining part of the fire keeps on that part of the glaſs which leaves the hand and goes to the Chain. III. Of that reſidual part on the ſurface of the glaſs a portion gets into the Chain, where the glaſs paſſes neareſt; which portion increaſes till it has attained a ſame ratio to the capacity of the Chain, as the portion which remains has to the capacity of the glaſs.*

149. That the electric fire paſſes from the hand to the glaſs at the place where the friction is effected, we ſhall find manifeſt when we

we confider that this glafs is the fole moving principle of the contrary electricities in the apparatus; that fuch electricities are excited with a greater vivacity, as the friction is performed in a better manner and with a properer degree of force; and, laftly, when we fee that thofe parts of the hand which actually performs the friction, are found to be electric by deficiency; and that thofe parts of the glafs which have juft left the hand, are found to be electric by excefs. Let the circle *a b c d* * defign the equator of a cylinder rubbed by the hand B: now, whenever a point is prefented to that equator, a *ftar* appears on that point; but whenever the fame point is prefented to the hand (the Machine being infulated) a *brufh* appears upon it. Nor are we to wonder that in the common way of making experiments † no light appears on thofe parts of the glafs where the friction is effected; this is becaufe the electric fire, in diffufing itfelf from the hand into the glafs immediately contiguous to it, meets with no refiftance; now, a refiftance is the only caufe which ever makes the electric fire become vifible.

150. Laftly, that the fire which from the rubbing body paffes into the glafs, is proportioned to the quantity which is able to be actually fupplied from elfewhere to the faid body, is manifeft from all that has been faid concerning the laws of *exiftence*, and of *connection*; and this I moft fatisfactorily fhew, by rubbing the cylinder or globe with the cufhion reprefented in Pl. I. Fig. 8. which is infulated with a ftick of glafs: in this cafe the cylinder or globe fcarcely exhibit any electricity.

151. With regard to the other parts of the law, objects of this chapter, they are rendered manifeft by the means of the three following lights. I. By the means of a feries of little ftars which fparkle in *e* (Pl. III. Fig. 2.) on the inferior extremity of the rubbing fingers, that is, where the glafs, which is fuppofed to move according to the letters *a b c d*, begins to part from the hand; thofe ftars manifeft that portion of fire which, in that place, gets back into the hand; and I call fuch light, the light of *feparation*. II. By the means of the ftar that appears on the point of the

* Pl. I. Fig. 1. 6. Pl. II. Fig. 2.

† We fhall fee afterwards the reafon why there appears a light, when the cylinder, or globe, are emptied of air.

Chain (Pl. I. Fig. 7.) which ſtar I call *light of diſtribution*, as that light indicates that portion of the electric fire which from the glaſs diffuſes itſelf into the Chain, proportionably to the capacity of it. III. and laſtly, By the means of another ſeries of ſtars, which appear likewiſe on the hand in *f* (Pl. III. Fig. 2.) where the glaſs in revolving returns to the hand: theſe ſtars, which I call the light of *return*, indicate that portion of fire which remains on the glaſs, after it has paſſed near the Chain.

152. That thoſe three lights muſt be conſidered as true ſtars, is rendered manifeſt by the following transformation I make of them. Inſtead of a cylinder of glaſs, I now rub a ſolid cylinder of ſealing wax, and the reſult is, that inſtead of ſeeing on the fingers in *e*, and the hand in *f*, ſhort flattened lights, we now ſee long tracts of light take place from the fingers in *e* and from the upper part of the hand in *f*: thoſe in *e* follow the wax as it moves away; thoſe in *f* throw themſelves out to meet the wax as it returns to the hand: likewiſe, inſtead of the ſtar which before appeared on the Chain, there now ſprings from it a moſt conſpicuous bruſh. All this is to be explained thus: the wax ſtick (this property of wax we ſhall prove more fully afterwards) gives to the rubbing hand a part of its fire, whereas the glaſs draws fire from the hand; and the hand, which is then emptied of its own fire, becomes apt to receive it back from the overloaded glaſs; both when this eſcapes from the hand, and when it returns to it ſtill loaded with exceſſive fire: but as this ſame hand, when the experiment is made with wax, itſelf becomes overloaded with fire, it then throws it towards the wax, as it moves away deprived of its own, and towards that ſame wax when this comes back to it, ſtill deficient: and as the overcharged glaſs diffuſed before its exceſſive fire into the Chain, ſo now does the Chain throw its own into the wax, which, by being rubbed, is, in conſequence of its peculiar nature, become deficient.

153. But with regard to the light of *ſeparation*, and that of *return*, they are made to exhibit a more preciſe appearance of a ſtar, and of a bruſh, by performing the friction with a piece of gilt paper; with this obſervation, that the glaſs muſt be rubbed with the gilded ſurface, and the wax with the naked ſurface of ſuch paper. When the friction is executed on glaſs, the edges of the paper exhibit

Tab. V.

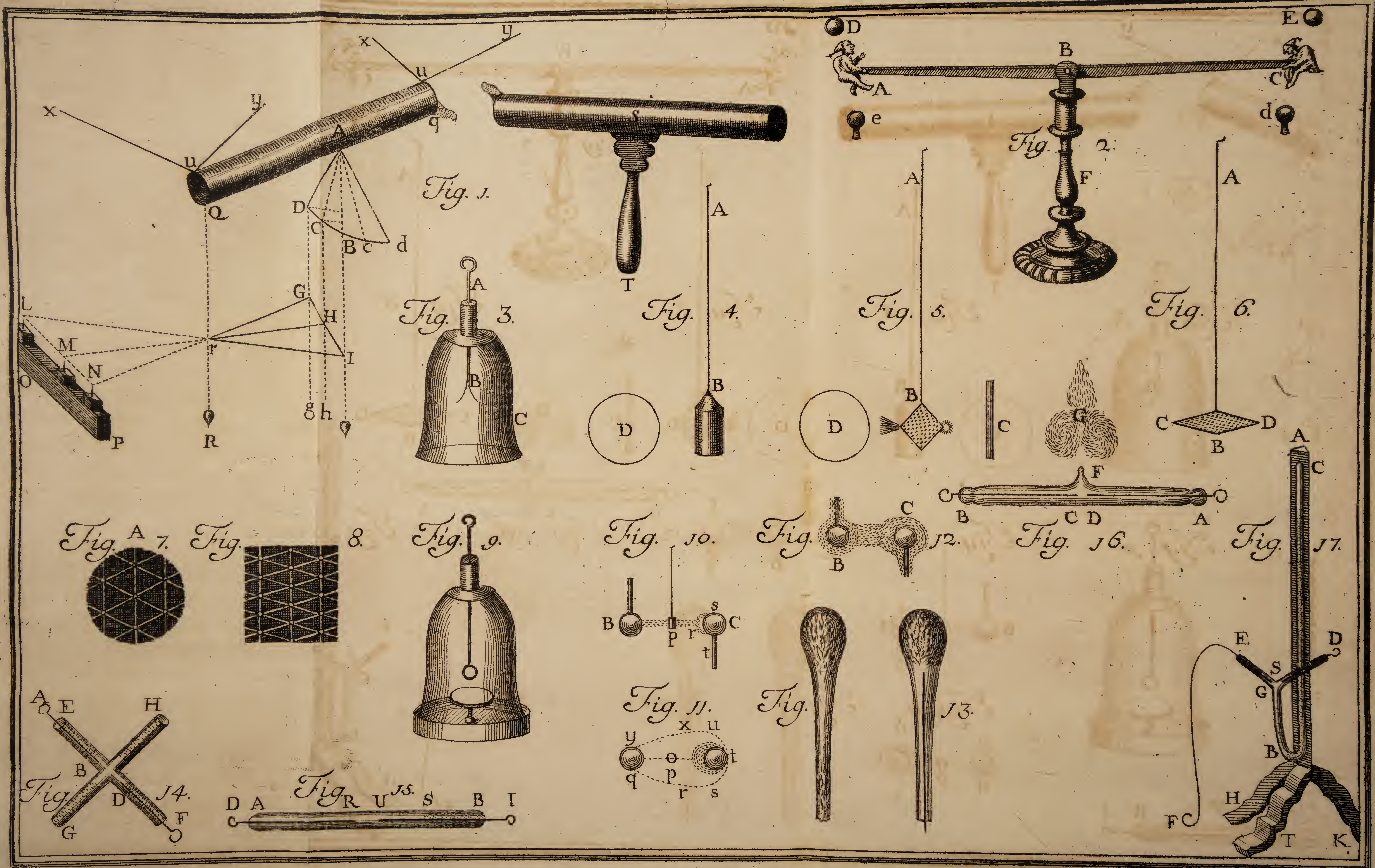

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Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17.

hibit a ſeries of moſt conſpicuous *ſtars*; and, on the contrary, throw moſt vivacious *bruſhes* when the experiment is made on wax. Likewiſe, if the point which the Chain offers to the glaſs, or to the wax, be ſharper than uſual, then both the appearances of a bruſh, or of a ſtar, will be more ſtriking.

154. I have hitherto ſpoken of the three lights jointly: I muſt now ſay ſomething on the multiplication that may be effected of the lights of ſeparation and return. When I rub with my hand, or with any other ſuch body as does not touch in a continued manner the ſurface of the rubbed body, a light appears in all ſuch intervals as are formed between thoſe parts into which the rubbing body happens to be divided, for inſtance, between my fingers, if I rub with my hand. Now, certainly, all theſe lights are nothing elſe than lights of *ſeparation* and *return* together: where the glaſs ceaſes to be in contact with one of the fingers, a part of the fire left by the ſaid finger, gets back into it, and another part of the ſame fire throws itſelf into the next finger, with which the glaſs is again in contact. This obſervation on thoſe lights, beſides its neceſſarily belonging to the ſubject, is moreover uſeful in this, that it ſhews that in exciting the electricity by a friction, there is no neceſſity to perform the ſame with a continued ſurface.

155. Thoſe operations being once made, there is ſcarcely any thing elſe to obſerve with regard to the light of *ſeparation* in particular, but that its exiſtence and vivacity correſpond both to the capacity of the Machine, and the efficacy of the friction. If the Machine be inſulated, the light of ſeparation will appear for as little time only as will be ſufficient for the fire in the Machine to be carried into the Chain; this done, the rubbing hand can no more afford to depoſit fire on the ſurface of the glaſs; therefore no more fire can afterwards flow back from the ſame into the fingers; and thus there will be no more a light of *ſeparation*. All which confirms the ſecond part of the law.

156. With regard to the third part of it, it is rendered equally manifeſt by the light of *diſtribution*; or otherwiſe, by that ſtar which appears on the point of the Chain, where the rubbed glaſs paſſes neareſt. The exiſtence and vividity of it correſpond, both to

the capacity of the Machine to give fire to the glaſs, and to the capacity of the Chain to receive the ſame. Thus, if I rub the glaſs with the inſulated cuſhion as in Pl. I. Fig. 8. we can hardly perceive for an inſtant, and even in a dark room, a moſt weak light upon the point of the Chain. If I rub with my hand, ſtanding on the inſulated Machine, the light will appear ſomewhat longer; but now it will be found perpetual, if I rub from the ground, and if, beſides, the Chain alſo communicates with the ground: all this is becauſe, in the latter caſe, the Machine can perpetually give, and the Chain perpetually receive fire. But however ſtrongly I may rub from the ground, when the Chain is once inſulated, the ſtar will not appear upon it any longer than the ſhort time which is neceſſary to ſaturate it. I increaſe the capacity of the Chain, by annexing to it one or more phials to be charged; then the ſtar appears the longer as more time is become requiſite to complete the ſaturation; and the vivacity of the ſame is continually and gradually decreaſing as the ſaturation gradually draws nearer to its completion. Whence there can be no doubt but that the ſtar is formed by fire which from the glaſs diffuſes itſelf into the Chain.

157. All this is no leſs evident with regard to ſuch fire as is accumulated by the friction, on that part of the glaſs which from the hand proceeds to the Chain: the following experiment will demonſtrate it. Let the glaſs be revolved in the direction *a b c d* (Pl. III. Fig. 2.) and the point of the Chain be placed near *b*. If, things being ordered ſo, I preſent a bruſh made of metal wire to any point *a b* of the glaſs, in that part of it which proceeds to the Chain, then there is no longer either ſtar or other ſign of electricity on the Chain; but if with the ſame bruſh I glide ſlowly along that part *c* of the glaſs which has already paſſed the Chain, then the latter exhibits, as uſual, the ſtar and other ordinary ſigns. If I make the glaſs turn in a contrary way, the ſame things will alſo take place when I apply, likewiſe in a contrary manner, the metallic bruſh. Univerſally, in order to have the ſtar appear on the point annexed to the Chain, and the Chain charged in conſequence of that ſtar, it is requiſite that the portion of fire on the glaſs do not meet, before it reaches the Chain, any other body into which it may diffuſe itſelf.

158. Laſtly,

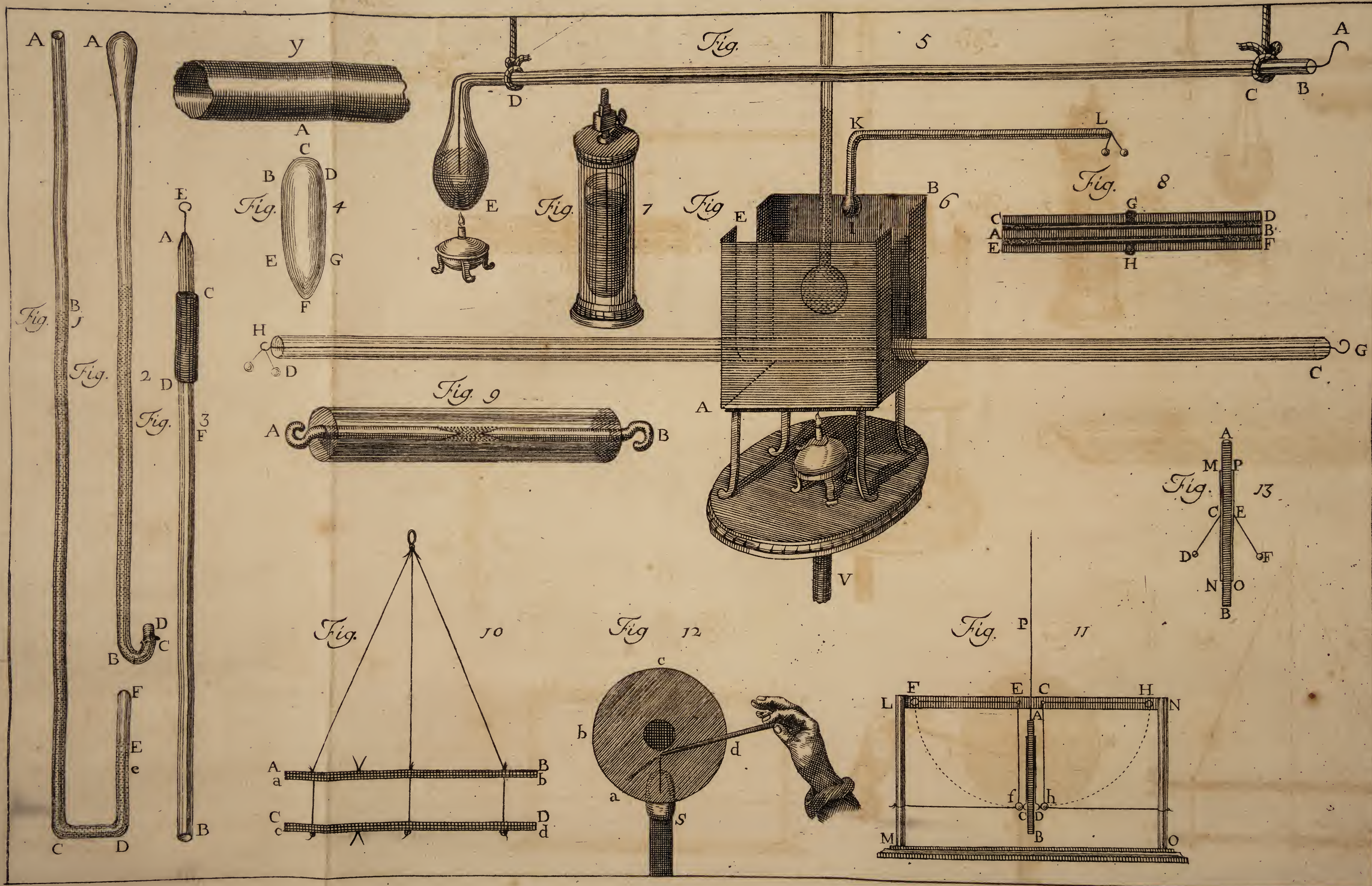
158. Laſtly, the light of return correſponds, with regard to its exiſtence and vividity, to the capacity of the Machine to give fire directly, and to the capacity in the Chain to receive it, inverſely. That is to ſay, according as the whole, or only a portion, of the fire on the glaſs gets into the Chain, none, or a leſs part of it, remains on the glaſs there to form the light of *return*. Thus, if the Chain communicates with the ground, it then can admit any quantity of fire the glaſs may carry to it; conſequently, little or no fire will remain on the latter to form the light of return. If the Chain be inſulated, the light of return ſpeedily returns, and ſoon increaſes its vividity, the Chain being then ſoon ſaturated with electrical fire: if, in the mean time, a ſpark is drawn from the Chain, then the ſtar begins afreſh to appear upon its point, and the light of return on the glaſs again fails. Let afterwards a large jar of glaſs be annexed to the Chain, in order to be charged; the light of return will then be continually increaſing, in proportion as the ſtar on the point of the Chain will be continually deadening: that is to ſay, the light of return will decreaſe in proportion as the charge of the Chain, (of which the capacity is become larger on account of the annexed jar) draws nearer to its completion. In ſhort, the light of return, otherwiſe the fire remaining on the glaſs, when from the Chain it goes again to the hand, is to the light of *diſtribution*, that is, to the fire which from the glaſs goes to the Chain, as the capacity of the glaſs, is to the capacity of the Chain.

159. Thoſe three lights therefore, viz. the light of ſeparation, the light of diſtribution, and the light of return, demonſtrate with the utmoſt evidence, the whole tenor of the two laſt parts of the law of *excitation*; but it is proper here to mention ſome accidents of that light itſelf, which might create ſome difficulty to new obſervers. The firſt is, that when inſtead of uſing the point of the Chain for exhibiting the ſtar, I uſe a bruſh of metal fixed to it, then long and extenſive bruſhes of fire ſpring from ſeveral of the threads or wires that compoſe it. But if we conſider the circumſtances and the directions of theſe bruſhes, we ſhall diſcover their real cauſe: they only ſpring out when the Chain is fully charged; they only ſpring from ſuch wires, or threads, as may

direct-

direct themſelves towards the Machine, and theſe threads really direct themſelves that way: that is to ſay, the exceſſive fire in the Chain directs the moveable parts annexed to it towards ſuch neighbouring bodies as are not electrified, and much more ſo towards ſuch bodies as are contrarily electrified, as for inſtance, is the Machine when inſulated: no wonder therefore, that ſuch fire, ſince it actually ſprings from thoſe moveable parts of the Chain, exhibits the appearance of a bruſh. From all this we may conclude what is the beſt way of ſuppreſſing the ſaid bruſhes; let firſt the metallic threads be kept fixed and directed towards the rubbed glaſs; ſecondly, let a part of the glaſs be rubbed, broad enough to make the fire ruſh from it, even laterally, to every part of the ſaid threads, and it will thus reſtrain the fire in them from diffuſing itſelf.

160. The other accident is ſtill more ſurpriſing at firſt, but proceeds from the ſame cauſe: when a globe, or a cylinder, or a plate of glaſs, are rubbed with cuſhions covered with amalgam of tin and mercury, ſtrong ſparks are often ſeen to break between the Chain and the hand, and plentifully to run around the circumference of the glaſs: theſe are owing to ſuch particles of the amalgam as happen to ſpread themſelves on the ſurface of the glaſs, and there effect a pretty continuous ſeries of deferent points, through which the electric fire, accumulated to a great denſity in the Chain, diſcharges itſelf from it, and throws itſelf into the cuſhion. The fire, beſides, depoſited on all the ſurface of the glaſs, may co-operate in the ſaid accidents: it may cauſe a ſpark to be continued; or it may open ſome little paſſage through which all the fire of the Chain, which proportionably to its denſity endeavours to ſpread itſelf, may afterwards be diſcharged; ſo that this fire, being thus tranſported from place to place, is carried back to the hand or cuſhion. The accident might be productive of ſome bad conſequences, if the friction being effected with the hand, a diſcharge were to take place in the very time that many large jars are juſt completely charged: in order to avoid ſuch accident, care muſt be taken not to have that part of the glaſs which is rubbed, exactly poliſhed; thus the ſeries of deferent points never can be completed: beſides, the hand muſt be kept at as great a diſtance as can be from the Chain; it even will be better to rub with a cuſhion.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APPENDIX.

161. Of two bodies whatſoever, A and B, ſo ſituated as to be eaſily brought to be rubbed, the one by the other, the one *v. g.* the body A gives to the other its natural fire and all other that may come and ſucceſſively ſupply the defect of that which it has already given: in this manner, the excitation of two contrary electricities is preſerved; viz. of an electricity by deficiency in A that gives, of an electricity by exceſs in B that receives the electrical fire. But why, in any given caſe, does the one of the bodies, *v. g.* A give its fire to B, rather than B gives its own to A? The ſolution of this queſtion would be an excellent and moſt luminous complement to this chapter.

162. But in caſe we could not ſucceed in reſolving that difficulty, I think it will not be amiſs to preſent here ſome propoſitions that may at leaſt procure ſome approximation towards the ſaid ſolution. I obſerve, firſt, that *of two bodies of a ſame or ſimilar nature, that which rubs gives electric fire to that which is rubbed.* I mean by the body that rubs, that which moves on a ſame part of its ſurface ſucceſſively along ſucceſſive different parts of the ſurface of the other; which latter, I conſequently call the rubbed body.

163. The firſt experiment that led me to that important truth has been made on a ſtocking made of black ſilk, which I had cloſely wrapped around a cylinder of glaſs (Pl. III. Fig. 12.) and tied with ſtrings of ſilk *a h*, *a b*, *c f*, *c d*, and ſtitched together in *a c*: I have rubbed it while the ſaid cylinder was turning, with the fellow-ſtocking to it of black ſilk, when I have ſeen an exceſs riſe in the Chain, and a deficiency in the Machine.

164. I ſoon after ſubſtituted a ſtocking of white ſilk to the black one, and rubbed it with the fellow to it, of white ſilk, when the ſame effect has reſulted.

165. I ſubſtituted a band of black velvet, and in rubbing it with a piece of black velvet, I ſaw likewiſe ſparks by exceſs take place on the Chain. I ſubſtituted a band of white velvet (all theſe bodies were applied after the ſame manner to the cylinder, Pl. III. Fig. 12.) and in rubbing it with a piece of white velvet, I perceived the ſame effects.

166. I

166. I then proceeded to try if the ſame would alſo take place in making the experiment, as I have done ſo often, with the ſkin of a cat. Having applied this to the cylinder, I rubbed it with a piece of ſkin of the ſame kind, when I obtained ſparks by exceſs from the Chain, as ſtrong, at leaſt as any I could excite by rubbing with my hand.

167. I have tried the ſame experiment with glaſs, by rubbing a cylinder of glaſs pretty ſolid, ſometimes with a tube, and at others with a ſtick of glaſs; but in whatever manner I might preſs upon the glaſs, I never ſucceeded to perceive the leaſt electricity ariſe in the circle traced by the tube or ſtick, on the cylinder while it turned; however, when I preſſed more ſtrongly, the two glaſſes then began to grind each other; and there aroſe, in the place of the friction, a little flame of a golden reddiſh colour.

168. I imagined that the friction in this experiment produced no electricity, becauſe it was performed with too much force and deſtroyed the glaſs: I tried to moderate it. I fixed two ſticks made of ſealing-wax to the centres of two thin poliſhed round plates of cryſtal, about five or ſix inches in diameter: then, with the help of theſe two ſticks, I preſſed the two uncoated ſurfaces againſt one another, and being thus rubbed, a degree of electricity reſulted in both. When I afterwards attentively examined in what manner the friction had been performed, I conſtantly found that that plate which with its moſt prominent parts deſcribed the largeſt circles or lines on the ſurface of the other, grew electrified by deficiency, and the other by exceſs.

169. I then tried to experiment with ſealing-wax. I took two ſticks of a ſame ſize, and with a ſame part of the ſurface of the one, I moved along the ſides of the other, which, at the ſame time, I was ſoftly revolving on its axis: when I found that the ſtick with which I had rubbed the other, had contracted in the end an electricity by deficiency; and that which had been *rubbed*, electricity by exceſs.

170. Seeing how conſtantly the above principle obtained, when the natures of the bodies were *the ſame*, I tried if it would prove equally true with regard to bodies of only *ſimilar* natures, by rubbing hair of a kind of animals, with hair of animals of another kind;

kind; and making uſe of hair of a certain fineneſs, I have always found the *rubbing* body to give its own fire to that which was rubbed *.

171. The reſult of thoſe experiments is, therefore, that of two bodies, A and B, of a ſame, or very ſimilar nature, A which in a given time undergoes the greateſt friction, will give its fire to the other. Let us ſuppoſe the caſe mentioned above, of a band of black velvet wrapped around the cylinder, and rubbed with a ſimilar piece of black velvet: the friction which the latter piece undergoes, is to that undergone by the former, as the revolving circumference covered by the ſaid band, is to the arch occupied by the piece of velvet by which it is rubbed: ſo that if the revolving circumference is ſuppoſed to be $= 360^{\circ}$, and the ſaid arch $= 15^{\circ}$, the friction ſuffered by the rubbing piece will be to the friction ſuffered by the rubbed band, as 24 is to 1; that is, when the band is rubbed once, the piece is rubbed twenty-four times.

172. But, trying afterwards to make experiments with ſubſtances of different natures, I found that the above law, viz. that the body which ſuffers the greateſt friction, gives its fire to the other, obtained no more: *there are, on the contrary, certain ſubſtances which, even when undergoing a much leſs degree of friction than others, yet love to give their fire to them.* Of this kind are reſins, ſulphurs, oleous, oiled, or even varniſhed ſubſtances, and the various kinds of ſilk, eſpecially thoſe dyed black. When I rub with my hand, with paper, or with a piece of oiled ſilk covered with amalgam, a ſtick of ſealing wax, or of reſin, or of ſulphur, or a ſtick of wood varniſhed with oil of linſeed, let ſuch friction be performed in any manner it will, the reſult will nevertheleſs conſtantly be, that thoſe bodies, inſtead of giving fire to the hand or the paper by which they are rubbed, will draw the ſame from thence: and if the friction is performed on a ribbon of black ſilk, the effect will ſtill prove the ſame, and the ribbon will alſo draw fire from the

* I had already made all the above experiments, when an experiment made after the ſame manner by Mr. Bergman, came to my knowledge (Phil. Tranſ tom. 54. § 3.) He electrified by exceſs a white ribbon, by rubbing it, in its whole length, ſucceſſively, againſt a given part of another white ribbon.

ſaid hand or paper. Laſtly, the ſame piece of glaſs which, as long as it continues poliſhed, receives electric fire from other bodies, gives its own, when it is once ground with emery, and rendered rough; as Mr. Canton has firſt obſerved. I have a tube of cryſtal A B C (Pl. III. Fig. 13.) poliſhed in A B, and rendered rough with emery in B C; with a ſingle friction I electrify it by exceſs in A B, by deficiency in B C.

173. But with regard to the property of certain bodies, of giving their own fire, we may lay down this in ſome degree univerſal law, viz. *that metallic bodies have in themſelves, in an eminent degree, this property, that when they are rightly managed they can give their own fire, and electrify by exceſs, ſuch other bodies as would in other caſes give theirs, and be electrified themſelves by deficiency.* I apply to a Machine the cylinder of wood A B (Pl. III. Fig. 8.) encompaſſed all around with a very thick band of ſealing-wax, or ſulphur: then, with the ſame gilded card, I can electrify either that band, or the Chain, which ever I will, either by exceſs or deficiency; viz. if I rub with the naked ſide of the card, the Chain will be electrified by defect; if I rub with the gilded ſide, the ſame will be electrified by exceſs. Oppoſite kinds of electricity are alſo produced on a ſtick of ſealing-wax, by rubbing it alternately with the naked ſide of the card, and with that which is gilded or ſilvered. If I afterwards rub the ſaid ſtick of ſealing-wax, with the ſilk cloth covered with amalgam, the wax becomes electrified by exceſs, with as much vivacity as it ever is electrified by deficiency, when rubbed with bodies of another kind. Likewiſe, however a black ſilk ribbon may love to give its own fire, yet when rubbed with the ſilk cloth covered with amalgam, it acquires a conſiderable degree of electricity by exceſs: even the black velvet, even the unpoliſhed glaſs, when rubbed with the amalgam, become electrified by exceſs. And here we muſt mention again the uſefulneſs of the amalgam in procuring a ſtrong electricity by exceſs in bodies, ſuch as glaſs and poliſhed cryſtals, which, of themſelves, are ſo much inclined to it. To the ſame cauſe I impute the ſtrong electricity I can excite in the Chain, by turning the plate A B (Pl. I. Fig. 10.) in mercury (§ 35.) And if the plate might be turned with a greater degree of velocity, without

without ſcattering the mercury, I ſhould prefer that method of exciting electricity to any other: thus all trouble with regard to ordering the cuſhions and keeping them clean, would be avoided, as well as the neceſſary care to make them preſs againſt the plate ſufficiently and uniformly; which circumſtance I do not know if it ever can be, after all, ſo well obtained as it is with mercury; which, in conſequence of its weight, continually preſſes againſt the plate.

174. I ſhall finiſh with relating how by rubbing with a metallic ſurface, I have changed into an electricity by exceſs, the ſtrongeſt electricity by deficiency that ever was excited. I procure the latter electricity by rubbing a band of black velvet, applied round the cylinder (Pl. III. Fig. 12.) and entirely freed from moiſture by the help of the ſkin of a wild cat, made very warm; I obtain from that velvet an electricity by deficiency in the Chain, as ſtrong, at leaſt, as any electricity by exceſs I can raiſe in it by the help of the cylinder, globe, or plate; which, by the by, ſhews that the electricity by deficiency does not differ only in degree of intenſity from the electricity by exceſs. But afterwards, to return to our object, if I rub the ſame band of black velvet with the amalgamated cloth, that powerful deficiency in the Chain is gradually turned into a pretty conſiderable exceſs.

175. I ſhall propoſe here ſome queſtions. May not we impute ſuch a propriety in metals, of giving their own fire, to their being deferent in the greateſt degree? May not we impute to the ſame principle the increaſe of efficiency in the cuſhions or fingers that rub the glaſs, when they have been previouſly made wet, and that an humid vapour, (as being deferent) like metals, increaſes electricity? We ſhall ſee afterwards that the common fire propagates itſelf with more eaſe through ſuch bodies as conduct eaſily the electric fire, than through ſuch as poſſeſs that quality in a leſs degree; and that the electrical fire produces with the greateſt rapidity thoſe very ſame effects which the common fire is uſed to produce ſlowly. May it not be that thoſe two kinds of fire only differ in their degrees of purity; that is, that the electrical fire is free from thoſe particles of other ſubſtances within which the common is in a manner impriſoned? A rubbing body contracts more

heat than a rubbed body; may not the reafon be that the body that rubs actually diffufes its own fire into the other? I once repeated the ingenious experiment of Mr. Bergman, by moving the fame part of a white ribbon, not warmed, along the furface of another white filk ribbon, ftretched over lighted charcoal, and confequently warmed to a great degree, when the rubbing ribbon, which was cold, became electrified by excefs; the rubbed ribbon, which was warm, became electrified by deficiency. Is not the inflammability of refins, fulphurs, and other oiled fubftances, the caufe why fuch bodies are inclined to be electrified by deficiency, fince in confequence of fuch inflammability they receive, when rubbed, a greater degree of heat? And may not the great quantity of phlogiftic with which metallic fubftances abound, as well as the great eafe with which common fire moves within them, be the reafon why metallic fubftances quickly contract by a friction, a confiderable degree of heat, and are thus difpofed to give their electric fire in the higheft degree? We fhall fee that when metallic bodies are melted by lightning, the bodies near them are much expofed to be burnt by the phlogiftic which is then freed out of them: we may add to the above conjectures, that glafs, when unpolifhed, alfo contracts heat more eafily on account of the feparation between the parts which compofe its furface, and of their fmallnefs; in the fame manner as inflammable bodies are the more eafily kindled, as their furface has a greater ratio to their mafs.

SEC-

SECTION II.

On the Theory of inſulating bodies, with regard to the charging and diſcharging of them.

176. I Confine the ſubject of this ſection to the charging and diſcharging of inſulating bodies, as the treating of thoſe different operations will be ſomewhat long; and beſides, I muſt treat ſeparately of the particular property of the *vindicating* electricity; a ſubject which I have amply explained in the book entitled, *Experimenta atque obſervationes quibus electricitas vindex conſtituitur, atque explicatur.* Taurini, 1769: nor can I give a convenient abridged expoſition of that property, before I have treated of the different kinds of electrical motions.

CHAP. I.

In which the theory of inſulating bodies, with regard to the charging and diſcharging of them is explained, and proved by the help of the bruſh and little ſtar, on plates of glaſs eſpecially; and is extended to pieces of glaſs of various ſhapes, and to other inſulating bodies.

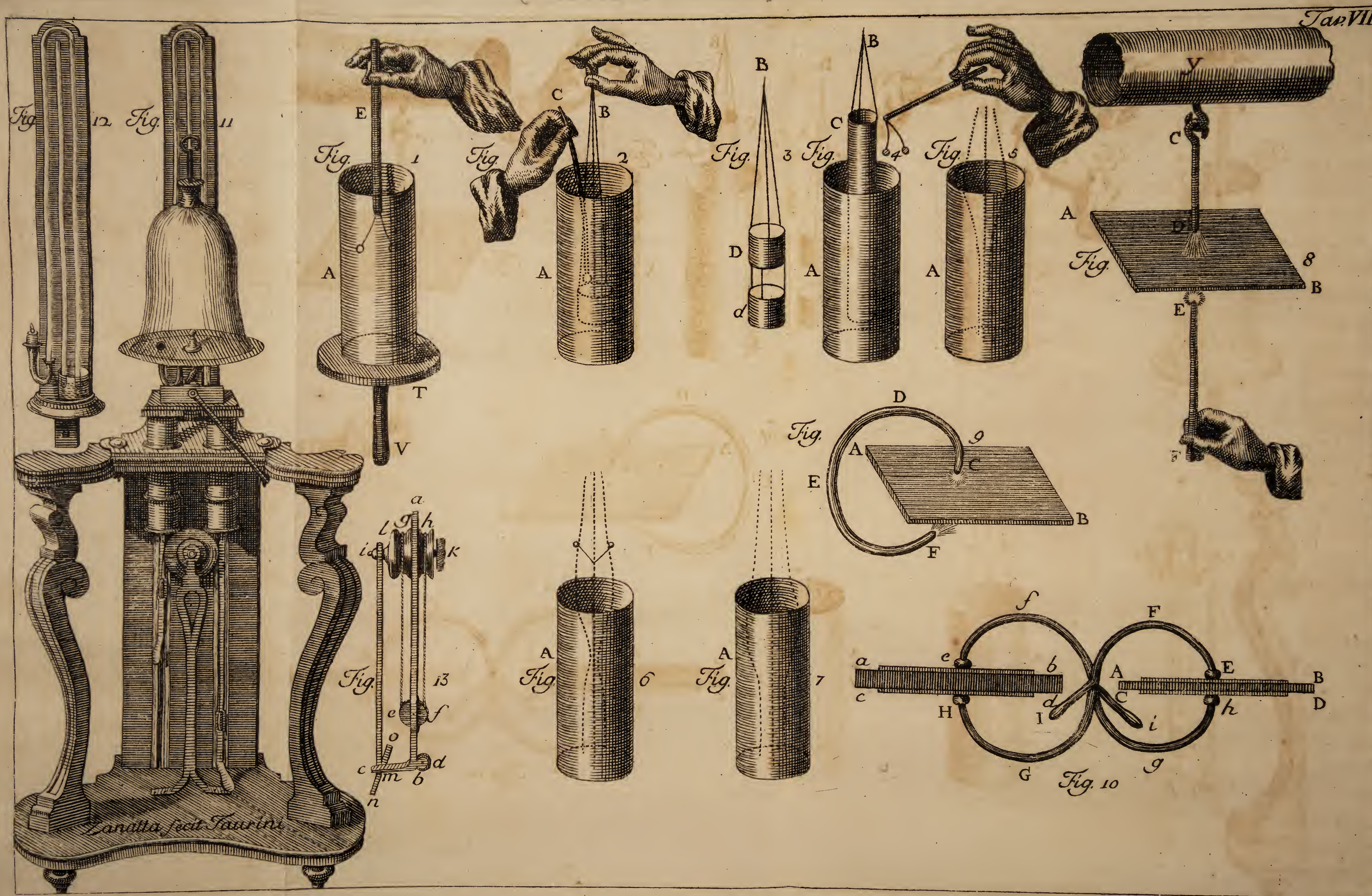
177. IN the year 1746, Mr. Muſchenbroek endeavouring to increaſe the electricity of deferent bodies, by ſurrounding them with ſuch bodies as might moſt exactly inſulate them, put ſome water (a body which is deferent to a conſiderable degree) into a bottle of glaſs; the latter body being much more compact than the air, was conſequently much more capable of inſulating the water: then the ſaid gentleman formed, with a rod of metal, a communication between the Chain and the water in the bottle; he then graſped the bottom of the bottle with one of his hands, and no ſooner had he touched the rod which from the Chain was let into it, in order to remove the bottle, but he received a ſtrong ſhock, which was accompanied by a loud ſpark, which ruſhed to his hand from the ſaid rod. Such is the origin of the denomina-

tions,

tions, *experiment of Leyden*, and *bottle of Leyden:* ſince that time other phraſes have been uſed, as of charging glaſſes, that is, of introducing into them a ſtrong doſe of electricity; and of diſcharging them, that is, of taking from them the introduced electricity.

178. No later than the year 1747, Dr. Benjamin Franklin analyſed that experiment; and here is the ſubſtance of the theory he laid down. *I. A body, in order to its being capable of being charged, muſt be inſulated, impenetrable, ſufficiently ample, and ſufficiently thin.* This body muſt be inſulating, that is, of ſuch nature as not to allow the electrical fire to move from one place of it to another, either along its ſurface, or through its ſubſtance: it muſt be impenetrable, that is, beſides its being inſulating, it muſt be compact enough to keep the electrical fire from making its way from one of the oppoſite ſurfaces of it, to the other, either through the interſtices of the rare texture of it, like that for inſtance of a ſilk cloth; or through any ſmall fiſſure; or alſo through a mixture of deferent particles that might form on its ſurfaces a ſufficiently continued ſeries. Again, ſuch body muſt be ſufficiently ample, as all other circumſtances being equal, the quantity of the charge on a body muſt be proportioned to its ſize. And it muſt be ſufficiently thin, becauſe all other circumſtances being again ſuppoſed equal, the quantity of its charge will be proportioned to its thinneſs; though, it muſt be added, that ſuch thinneſs muſt not be neither ſo much to the detriment of the neceſſary ſolidity, as to allow the force of the charge to break the too weak ſubſtance of the body.

179. *II. In order to produce the charge, an ample communication muſt be eſtabliſhed between the two oppoſed ſurfaces of the inſulating ſubſtance (taking care that one of the ſaid ſurfaces communicates not with the other) and two different ſyſtems, the one of which, at leaſt, muſt be actually electrified.* Becauſe, the quantity of the two equal contrary electricities that will take place on the two oppoſed ſurfaces, and which conſtitutes that which we call the *charge*, will be proportioned to the quantities of the contrary electricities raiſed in the two ſyſtems jointly; or, if an electricity is excited in only one of the two ſyſtems, to the quantity alone of that electricity. Let D C repreſent the profile of a large plate of glaſs (Pl. IV. Fig. 1.) let *F a E* repreſent the communication between one


Tav.VII.
Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9
Fig. 10
Fig. 11
Fig. 12
Fig. 13
Zanatta fecit Taurini

one ſurface of the ſame and the Chain, and *H b G* the communication between the other ſurface and the Machine. The exceſſive fire which the rubbed glaſs tranſmits to the Chain, will be accumulated in a great quantity on the ſurface *F a E*; and, though it does not penetrate through the ſubſtance of the glaſs, will drive an equal quantity of natural fire from the other ſurface *H b G*. And, reciprocally, the force of the cylinder, by its drawing a great quantity of fire from the Machine, will conſequently draw a great quantity from the oppoſed ſurface *H b G* of the plate, annexed to it; and, at the ſame time, enable a great quantity of exceſſive fire to run from the Chain to the ſurface *F a E*: the concourſe of thoſe two forces will thus accelerate the formation of the two contrary electricities, and augment their quantity in proportion as the natural fire will be enabled to eſcape from one of the furfaces, and the exceſſive fire to accumulate itſelf on the other. If one of the ſurfaces *v. g.* *F a E* communicates either with the Chain, or with the Machine, and the other ſurface communicates with the ground, (which is the common way of making the experiment) the exceſſive fire that will flow into *F a E*, will ſuffice to drive the natural fire from *H b G*; or the natural fire that is extracted from the ſurface *F a E* (in caſe it is made to communicate with the Machine) will ſuffice to enable a quantity of exceſſive fire to run into *H b G*, but the operation will in that caſe be ſlower and leſs efficacious, juſt as we have ſeen, in § 122, the bruſh direct itſelf with a much greater force from the Chain to the Machine, than from the Chain to indifferent bodies, or from indifferent bodies to the Machine.

180. I muſt add, that a communication of the two ſurfaces with two different ſyſtems is abſolutely requiſite: *becauſe the exceſſive fire cannot accumulate itſelf on one of the two ſurfaces, without the oppoſed ſurface communicates with a ſyſtem into which it may, in the mean time, transfuſe its own natural fire: nor can one of the ſurfaces transfuſe its own natural fire, without the oppoſed ſurface communicates with a ſyſtem from which exceſſive fire may flow into it.* That is, the charging of an inſulating body is effected by raiſing at once, and in equal degrees, the two contrary electricities on the two ſurfaces of the ſaid body; either through the action of

two

two fyftems animated by contrary electricities, or through the action of an animated fyftem on one furface, while a communication remains eftablifhed between the other furface, and the indifferent fyftem.

181. *III. Laftly, in order to effect a difcharge, a communication muft be introduced between the two furfaces of the infulating body, when loaded as above with equal degrees of contrary electricities.* This is becaufe the two equal contrary electricities are no lefs dependent on each other, with regard to their being deftroyed, than as to their being produced: that is, as a quantity of exceffive fire cannot be introduced into one furface, but inafmuch as an equal dofe of natural fire can quit the other furface; fo *vice verfâ*, a quantity of exceffive fire introduced into one furface, cannot be taken off, but inafmuch as the natural fire taken from the other, is reftored to it. Confequently, as foon as a communication fhall be completed between the two furfaces of the infulating charged body, by the means of one or more continued deferent bodies, the exceffive fire will burft from that furface on which it was accumulated, and rufh to the other furface which was deprived of an equal portion of natural fire; and thus the infulating body will find itfelf difcharged, that is to fay, will find itfelf with its two furfaces reftored to their natural quantity of fire.

182. In fhort, the effect of a charge is to make the natural fire of the infulating body change its pofition without changing its quantity; any portion that may be added to one furface, is conftantly taken from the other; the fire in confequence of the difcharge does not decreafe, it only returns to its natural pofition.

183. Hitherto I have only had for my object to give an abridgment of the theory; I am now going to demonftrate it, by the help of the figns of the *brufh* and the *ftar*, as being the moft proper to render it evident. I. I hold by one of its angles, a plate of glafs A B (Pl. VII. Fig. 8.) which muft be thin and dry, and at firft only prefent it to the brufh D, which fprings with vivacity from the rod C D annexed to the conductor *Y*; when the brufh very foon ceafes and difappears: that is to fay, the electric fire thrown at firft by fuch brufh, cannot flow along the furface of the infulating glafs, nor go through its compact fubftance and get at my

my hand; nor again adhere to the ſurface of the glaſs but in a ſmall quantity, equal to ſuch portion as it may drive from the oppoſed ſurface of it, into the contiguous air.

184. II. But afterwards, if I preſent to the inferior part of the plate another rod *E F*, ſtanding in the ſame line with the point *C D*, the bruſh then revives on the point *D* of the rod *C D*; and, correſponding to it, the ſtar appears on the point *E* of the rod *E F*: that is, the exceſſive fire of the bruſh is now enabled to diffuſe itſelf along the ſuperior ſurface of the plate, and then unite with the natural fire in it; becauſe the natural fire of the inferior ſurface now can paſs into the deferent rod E F, and through it into the ground.

185. III. But it is to be obſerved, that if the two rods remain directed to the ſame part of the plate of glaſs, the ſtar and bruſh ſoon grow languid and fail. In order to revive them I muſt bring the rods nearer to the plate; but although they may at laſt become ſo near as to touch it, if they ſtill remain directed to the ſame part, all light will ceaſe to appear on the points. Whence it follows, that given portions only of electrical fire can be added to, and taken from, given oppoſed parts of the plate. One ſurface reſiſts the more againſt receiving new fire, as it has already received more; and the other ſurface reſiſts the more againſt giving its fire, as it has already given more: all which, if it is attentively conſidered, will be found conformable to the law of *diſtribution* (§. 86.) it is therefore very natural that while the points remain directed to the ſame oppoſite parts, the bruſh and ſtar, that is, the fire that flows to one ſurface, and that which leaves the other, ſhould firſt decreaſe and then fail.

186. IV. But if I move the plate horizontally between the two points, ſo as to make different parts of it ſucceſſively correſpond to them, the bruſh and ſtar again appear, grow languid and ſucceſſively fail; and, in ſhort, exhibit the ſame ſeries of appearances as before, relatively to all ſuch new parts of the plate as are ſucceſſively preſented to the points.

187. V. If in moving the plate, I bring it to ſuch a poſition between the points as to make the latter be very near the edge of it, then the bruſh of the point C D bends itſelf around this edge, as

if it ſought to avoid the glaſs; and from the edge bends itſelf again towards the point E F: that is, the electric fire declines going to the plate, and directs itſelf immediately to the other deferent point.

188. VI. If, after having by the means of the bruſh and ſtar, ſtrongly charged the greateſt part of the ſurface of the plate, I remove it from the rod C D, and preſent to either of its ſurfaces the point E F, the plate is not for all that diſcharged: for as the other ſurface remains inſulated, this ſurface cannot diffuſe away the fire with which it is loaded; it can do it only when the natural fire, of which the latter is deprived, ſhall be enabled to return to it.

189. VII. If, after having charged the plate by the means of the bruſh and ſtar, I preſent to the one ſurface of it, one end C (Pl. VII. Fig. 9.) of an incurvated metallic rod *C D E F*, and to the oppoſite ſurface, the other end F; then the plate is diſcharged, that is, loſes the fire of its ſuperior ſurface, which fire then forms a ſtar on the point C; and, after circulating through the rod C D E F, forms a bruſh on the other point F; thus reſtoring to the inferior ſurface the portion of fire that had been taken from it.

190. VIII. The law of the two ſigns appearing on the two ends of the rod bent towards the two ſurfaces of the charged plate when it is diſcharged, is the ſame (except that the ſigns appear in the contrary places) as the law of the ſigns manifeſted on the two points C D, E F (Pl. VII. Fig. 8.) when the ſaid plate is charged. That is, the ſtar does not appear on the end C (Pl. VII. Fig. 9.) but as long as the bruſh appears on the end F; nor does either of them appear but as long as the ends C and F are directed to the correſpondent parts of the two oppoſed ſurfaces. And with regard to given parts of thoſe ſurfaces, theſe ſigns gradually, and pretty quickly, grow languid and then fail; though they revive when the ends C and F are ſucceſſively preſented to other parts that have been alſo charged; and, laſtly, with regard to each ſeveral part, the vividity of thoſe ſigns deſtructors of the charge, correſponds to the vividity of the contrary ſigns with which the charge has been introduced into that part.

191. I repeat the experiment of introducing the charge into the plate A B by the means of the bruſh and ſtar; (Pl. VII. Fig. 9.)

then I place on the palm of one of my hands a ſheet of leaf-tin, like thoſe uſed for looking-glaſſes; and, holding the plate by one of its angles I invert it, placing it upon the ſheet. I then lay on the ſurface of the ſame hand another ſheet, equal to the firſt, and place it upon the upper ſurface of the plate. In ſhort, I apply the two ſheets in ſuch a manner to the two ſurfaces of the plate, that there be no communication between them; and conſequently, one ſurface only communicating with my hand, no diſcharge can happen (181.) But the ſpark burſts out and gives me a ſhock in the inſtant that I carry my other hand to the ſheet, placed on the upper ſurface of the plate. Then it is that the fire, introduced by the bruſh in the ſeveral parts of the under ſurface of the plate, burſts out at once, by the help of the deferent metallic ſheet applied to it, and runs through my body to replace at once into the upper ſurface, by the means of the other metallic ſheet, the fire that had been extracted from it.

192. And this ſhock which I receive from the plate clothed with the two metallic ſheets, and charged with the bruſh and ſtar, ſurely allows of no doubt that the electricity which produces it, does not lie in the deferent bodies joined to the ſurface of the inſulating bodies, but entirely in the ſurfaces of the latter; ſince both metallic ſheets had had, through my body, a communication with the floor, and could not conſequently have any electricity; it therefore remains, that they only tranſmitted the electricity introduced before into the plate, when naked.

193. It has only been in order to render the theory of the charge and diſcharge more manifeſt, that I have begun with propoſing the experiment of charging, and afterwards diſcharging, every ſucceſſive part of the naked plate by the help of the bruſh and ſtar; but in the common way of experimenting, there is no neceſſity for repeating theſe tedious operations. To both ſurfaces of the inſulating body deferent bodies need only to be adapted; theſe will completely ſpread the contrary electricities all over the ſurfaces, when the charge is forming; and in the diſcharge ſuddenly and completely gather the ſaid electricities. Thus, for inſtance, to the plate A B C D (Pl. II. Fig. 8.) let a ſheet of tin *f g h i* be affixed with a proper kind of glue;

let alſo a margin of two or more inches be left between the edges of the ſheet, and thoſe of the plate, on all ſides; let a ſheet of tin, equal to the former, be adjuſted to the correſponding parts of the other ſurface; and ſuch deferent bodies thus applied will at all times charge and diſcharge the plate, and are called the *coatings*.

194. In the preſent figure, the plate of cryſtal A B C D is raiſed on a round flat piece of wood, inſulated by the little columns of glaſs K L. When there is an occaſion for it, I place on the ground the iron boxes M, N, O, I, with hot aſhes in them to preſerve from moiſture the naked margins left on the plate. Then I form a communication between the Chain and any part of the coating *i f g h*; and, in order that the fire of the Chain may be tranſported to the ſuperior ſurface, I form a communication between the ground; or, what is ſtill better, between the inſulated Machine (179) and the *coating* of the inferior ſurface: and thus, in a little time, a great deal of exceſſive fire is accumulated on the ſuperior ſurface of the plate, while an equal quantity has ran away from the inferior ſurface: that is to ſay, the charge is completed. In order, afterwards, to effect a diſcharge, I make uſe of a rod of braſs O N M, terminated by two balls, likewiſe of braſs; I then bring the ball O into contact with the inferior *coating*, and when I preſent the other ball M to the ſuperior one, then a ſtrong ſpark (proportioned, all other circumſtances being equal, to the extent of the *coating*) is thrown out. I call the braſs rod, from the office I make it perform, the conducting bow; and a plate, thus armed or coated, is uſually called with us, the *Franklinian ſquare*; it being Dr. Franklin who firſt uſed one of ſuch a ſize to imitate the effects of lightning.

195. But in experiments that do not require ſtrong ſparks, there is no neceſſity of uſing ſuch ample plates, which cannot be charged but after a conſiderable time, and are not ſo manageable: I find that ſmall bottles of thin glaſs with a long neck, are extremely convenient; they are inwardly armed by being filled with water, or ſmall ſhot of lead; or by being coated inwardly with the amalgam we have mentioned before; the outward ſurface is coated, as uſual, with a ſheet of tin. I have repreſented two

ſuch

fuch bottles, infulated on the little ftool S (Pl. II. Fig. 6.) and four others armed with glafs handles, in the Fig. 3, 4, 5, 6, of Pl. III. The figure 5 of the latter, reprefents a bottle which is charging by receiving a brufh *c* from the Chain, and forming a ftar *c* on a point prefented from the ground B; and the 3d figure reprefents the fame bottle that is difcharged in forming the two fame figns in contrary places, on two points that communicate either with the ground, or between themfelves. The 6th figure expreffes a bottle armed with points, which receiving fire from the Chain in *c*, there exhibits a ftar; and in C gives the brufh to the ground, or the Machine; and the 4th figure reprefents the fame bottle when it is difcharged.

196. The law of the brufhes and ftars is ftill more diftinctly expofed in Pl. IV. Fig. 8. On the little bench O N Q P a point D is placed; and, at fome diftance, a little column of glafs is raifed, which infulates the large plate F F, armed with the two coatings H G, K I: the plate prefents two points B, C; the one to the little bench O N which communicates with the ground; the other to the conductor Q from which another point is directed to the coating. Now, at the fame time that the plate takes its charge from the Chain, the exceffive fire, which runs to the furface H G, forms a brufh, as it comes out of A; and a ftar as it gets into C: and the natural fire that is thrown away from the oppofed furface K I forms the brufh in B, whence it iffues; and the ftar in D, where it enters. But if after having completed the charge, I touch with one hand the conductor Q, and with the other the little bench O N, then the direction of the fire is inverted in confequence of the communication which I introduce between the two furfaces differently electrified (it is neceffary, in fuch cafe, that the points fhould be placed at a lefs diftance than that indicated in the figure) that is to fay, every fign becomes inverted. If, in order to charge the plate, its furface H G were prefented to the Machine, the figns, in the act of charging, would be contrary to the figns that appear when the charge is taken from the Chain, and would be the fame as thofe manifefted when a difcharge is made of a plate charged from the Chain; but if that fame plate were now charged from the Machine, a fimilar inver-

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ſion of the ſigns would take place, whenever it ſhould be diſcharged.

197. Any perſon who will repeat the experiments of charging and diſcharging naked or coated glaſſes by the means of the bruſh and ſtar, and will proceed with the neceſſary care and patience, ſhall be convinced by his own eyes of the truth of the propoſed theory. I flatter myſelf that I have ſufficiently demonſtrated the ſignificancy of both the bruſh and ſtar in Chap. V. of the preceding Section, I intend now to demonſtrate that experiment which I had reſerved for the laſt part of this chapter; that is to ſay, how other inſulating bodies (when ſufficiently thin and compact) may be, like the glaſs, charged and diſcharged.

198. In the year 1753, I charged a piece of native talc. I took a ſheet of it (Numb. 472 of the *Artificial Electriciſm*) as ſmooth as I could find it, tranſparent in all its parts, and free from furrows, or any opaque vein; I coat it in the ſame manner as a little Franklinian ſquare, and charge it in the ſame manner. When I touch one of its ſurfaces with one of my hands, and the other ſurface with my other hand, a diſcharge is effected, and I receive a ſmall ſhock.

199. I have ſpread ſealing-wax upon a piece of even poliſhed marble, anointed with oil of olives, and heated underneath by a fire not very violent (Vide Numb. 149. of the *Terreſtrial Atmoſpherical Electriciſm*); the ſealing-wax was ſpread in ſuch a manner as to form a thin even coating on the marble without any interruption or crack: I then was able to charge and diſcharge it, in the ſame manner as the Franklinian ſquare.

200. In Numb. 151 of the ſame book, I was ſtruck in the ſame manner by a coating of ſulphur, ſimilar to that in the preceding paragraph. In Numb. 155 I was ſtruck, though leſs ſtrongly, by a coating of pitch alone. In Numb. 156, I was ſtruck by a coating made with a compound of equal parts of roſin and pitch. In thoſe experiments I not only convinced myſelf that all inſulating bodies poſſeſſed the ſame propriety as the glaſs, but made beſides, two obſervations. I. That though ſulphur and reſins receive from friction an electricity different from that of glaſs, however, when they are electrified by communication, they

they are electrified in the same manner. II. That the least crack, or fissure, is enough to hinder the charging of the above mentioned coating. I had armed, with a gilded card, a coating of sulphur, and could only obtain from it a very small spark; but then, when I was forming the charge, I heard, from time to time, a spark that leaped from the one surface to the other; having found out the place, I cut the gilded card around the crack, which remained uncovered, and then the spontaneous sparks ceased, and I could obtain the convenient charge.

201. I increased afterwards the effects of those experiments by increasing the size of the instruments I made use of, as is mentioned in the collection of my new experiments, sent to the Royal Society the 14th of January, 1766. The Fig. 7. Pl. II. represents my fulminating table, described in the said collection. I. I cover the surface of the table with a sheet of lead, so as to have three inches of margin left marked; only a piece of the sheet gets out in E, and is bent towards the side of the table. II. To the four sides of the table, I fix four rules, which are raised above it about the twelfth part of an inch; so that there remains an empty parallelipepidal capacity, having for its base the surface of the table, and for its height the twelfth part of an inch. III. I melt a certain quantity of resin extremely well refined, and mix with it an equal weight of powder of marble, thoroughly heated, to exclude from it air and moisture. IV. I pour on the table this liquefied mass, and rolling a cylinder on the abovementioned rules, I level the surface and make it even: if, in cooling, some cracks or fissures are left, I close them with a hot iron. Lastly, to the surface A B of that substance, I adapt the sheet of lead *a b c d*, distant likewise three inches from the margin on all sides; and thus I have what I call the fulminating table; because if I make the metallic coating *a b c d* communicate with the Chain, the table becomes most strongly charged; and when I apply one head of the conducting bow to the part of the inferior plate of lead that gets out in E, and present the other head to the superior coating, a spark flies out more vehement than any thrown by a plate of glass of an equal size. Such tables as this may be made of all sizes, and have this advantage, that resins drawing less moisture

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than glafs, they are more completely infulated, and may be fufficiently charged, even in damp weather; but then they have this difadvantage, that through the viciffitudes of warm and cold weather, they become cracked in a thoufand places.

202. I have obtained ftill louder fparks from the fulminating globe, reprefented in Pl. IV. Fig. 5. To conftruct it, I take, in the firft place, a large ball, made of plates of fheets of brafs, two feet and two thirds in circumference, and this ball, or globe, ferves as an interior coating; then I cover the circumference GCBAEFGH, with a ftratum of refin mixed with powder of marble, and this ftratum is in its turn covered or coated all over the fpace C D A B C with a fheet of lead; fo that the body of the ball made of fheets of brafs, which ferve as an interior coating, remains unaccompanied by the exterior coating, in all the fpace occupied by the zone of cement E F G H A D C, which remains naked; I fufpend the ball by the hook H I, which I make communicate with the Chain, and after charging it, I difcharge it by applying one head of the conducting bow M L K in M, to the fheet of lead, and bringing the other head K to the hook I. The few fparks I have obtained from that fort of apparatus were extremely loud; more fo, I think, than could be obtained from glaffes of an equal dimenfion; but I have indeed drawn very few, every one of them confiderably damaging the cement in the place M whence I drew them, and the heat afterwards of the fummer has made it melt and run. The furface of the exterior coating of that ball, was about nine feet and a half. Befides its fize, the fhape of that ball, and fome other particular circumftances that I fhall mention hereafter, have contributed to increafe the charges; but I fhall fpeak more explicitly on that fubject in its proper place.

CHAP.

CHAP. II.

In which the ſame theory is confirmed by ſimple charges and diſcharges, made in different ways.

203. COMMON ſimple charges and diſcharges ſuffice, if duly attended to, to demonſtrate the manner in which the ſame are effected. I lay hold of the larger part of one of the bottles repreſented in Pl. II. Fig. 6. and apply the hook of it to the Chain; when I ſee the electroſcope annexed to the Chain diverge but ſlowly, and the more ſo, as the ſize of the bottle that is charging, is larger. When the electroſcope has attained its greateſt divergence, I preſent my other hand to the hook A of the bottle, and I draw a ſpark and receive a ſtroke, which are both the greater as the capacity of the bottle is greater; that is, according as more time has been ſpent in charging the ſame, and procuring to the electroſcope its greateſt divergence. Now, if the exceſſive fire had penetrated through the glaſs, it would have, as I held the glaſs in my hand, diffuſed itſelf through my body into the ground, and the diſcharge would not have had in the iſſue an intenſity proportioned to the time employed in charging; that is, to the ſum of fire that had been able to flow from the Chain during that time.

204. If, after charging the bottle, I place myſelf on an inſulated ſtool, I am likewiſe ſtruck, and there remains not either in the bottle, or in my body, any electricity; except perhaps a little remain of charge, otherwiſe an overplus of electricity, in the one of the coatings: of which accidents I propoſe to ſpeak more at large hereafter. Now, a given quantity of electricity never is ſuppreſſed by its paſſing into a finite capacity, but as it finds there an equal quantity of a contrary electricity; but there could be no ſuch electricity in my body, ſince during the charging it communicated with the ground. The only concluſion, therefore, we can draw, is that the exceſſive fire, in its diffuſing itſelf from the interior ſurface of the bottle through my body, finds in the exterior ſurface of it, an equal deficiency of natural fire, which it then entirely fills up.

205. The charges and difcharges made with naked or uncoated bottles, render ftill more evident the equality and reciprocal dependency of the two contrary electricities, that take place on the two oppofite furfaces of the infulating body. I touch the bellied part of a naked bottle with only two of my fingers, and I charge it in that manner. I charge it afterwards, by touching it with three, then with four, then with my five fingers. I charge it again by holding it with one hand, then by embracing it with both; and each of thefe charges takes more time to be completed, (which is known by the electrofcope taking more time to attain its greateft divergence) according as the exterior naked part of the bottle has been more amply touched; that is to fay, the exceffive fire does not accumulate itfelf on the interior furface, but correfpondently to thofe parts of the exterior furface which are touched, and through which, therefore, an equal quantity of natural fire can efcape.

206. The charges and difcharges made with fparks demonftrate with ftill more clearnefs the mutual dependence of the two contrary electricities, on the two furfaces. At a little diftance above the fuperior coating *f g h i* of the fquare A B C D (Pl. II. Fig. 8.) is fufpended a metallic rod, terminated in a ball; and meanwhile the fquare ftands infulated on the little column of glafs K L. In fuch a ftate of things, only a very little quantity of fire flows from the ball to the fquare; which is, as much as can pafs from the inferior furface of the fquare into its coating, and as much, befides, as is neceffary to produce in the fuperior coating an excefs equal to that in the Chain. Befides thofe fmall portions of fire, no other, I faid, flows from the ball to the fquare; but, afterwards, as often as I prefent my finger (which I always rapidly withdraw) to the inferior coating, two fparks leap in the very fame inftant, the one from the faid inferior coating to my finger, the other from the ball to the fuperior coating of the fquare. Thefe fparks are always equal to each other (I perceive better their equality when, inftead of my finger, I prefent to the inferior coating a ball of metal equal to that which hangs over the fuperior coating); but we muft obferve that the fucceffive pairs of fuch fparks, are continually decreafing, and, meanwhile, the divergence

vergence of the electroſcope increaſes. At laſt the ſparks fail, and I find in the ſquare a charge which is the ſtronger according as the ball was nearer to the ſquare; the reaſon of this laſt circumſtance is, that an electricity, though ſmall in the ſuperior ſurface of the ſquare, is ſufficient to reſtrain the electricity of the Chain, if the reſiſtance which the intermediate air alſo oppoſes to the latter, be much increaſed by the diſtance. Moſt certainly the above experiment demonſtrates that no quantity, even the leaſt, of electric fire, can accumulate itſelf on one of the ſurfaces of an inſulating body, but inaſmuch as equal portions can be thrown out from the oppoſite ſurface.

207. If the ball ſuſpended above the ſquare, communicated with the Machine, the experiment would furniſh exactly the ſame conſequence, except with regard to the direction of the ſparks, which then will be contrary to what it was in the former caſe: this alſo confirms that no portion whatſoever of fire may be taken (by communication) from the ſurface of a glaſs, but inaſmuch as an equal doſe is ſupplied on the oppoſite ſurface.

208. The diſcharge by ſparks ſpeaks no leſs eloquently. Let a ball of metal be preſented to the inferior coating of an inſulated ſquare: if the latter is fully charged, no ſpark can leap from the inferior coating to the ſaid ball; but if I touch, being inſulated, the ſuperior coating (I muſt obſerve to touch a ſtrange body when I have once touched the ſquare) two equal ſparks then leap at every touching; the one from the ſuperior coating to my finger, and the other, equal to the former, from the inferior coating to the ball.

209. I make the ſame experiments more expeditiouſly with a bottle A B (Pl. II. Fig. 5.) which I raiſe by the help of the inſulating handle E F: no ſpark leaps to the hook A, from the conductor C, but as I preſent my other hand to the bottle, and thus enable an equal quantity of natural fire to leap to it from the exterior ſurface of the bottle. And thus, by preſenting my finger at ſeveral times, ſparks after ſparks are excited, and the charge effected. The bottle being charged, I carry it to the Machine, which is animated by a contrary electricity; and I diſcharge it likewiſe by

correfponding fparks ; but thefe prove ftronger if they are formed by the exceffive electricity of the bottle, and the deficient electricity of the Machine together.

210. Befides this method of effecting charges and difcharges by the means of pairs of *fellow* fparks, there is another method, fimilar to the former, which I call charging and difcharging by alternation. Such difcharges confift in making the two coatings of an infulating body alternately touch two different fyftems, electrified in a contrary way; in each of thefe contacts fparks are produced, which are proportioned to the electricities of the two fyftems, directly, and alfo to the fums of the capacities of the two coatings; but inverfely to the fum of thofe fparks which have preceded them.

211. I hold by the help of an infulating handle F, the bottle A B, and rapidly move it from the Chain C to my finger I (Pl. II. Fig. 5.) which I keep at a fufficient diftance, fo that the fparks cannot leap, at the fame time, from the conductor C to the hook A, and from the bottle B to my hand; and at every contact between the Chain and my finger, diftinct decreafing fparks are thrown, which continue for a good while, but at laft become unperceivable : when this happens, I hold with one hand the bellied part of the bottle, with the other I take the hook, and then I receive a ftroke proportioned to the number of the fparks that have been excited.

212. I charge ftrongly the bottle, and holding it by the infulating handle, I alternately touch the hook and the bottle, and the confequence is, that equal fparks alternately burft out between the hook and my finger, and between my finger and the bottle: in proceeding thus with patience, thefe fparks will be found, firft to decreafe, and then totally to fail.

213. I infulate myfelf, and holding the bottle by the hook, I alternately touch the Chain with the faid hook, and a ftrange body, with the belly of the bottle; the bottle becomes charged by virtue of the alternative fparks which then take place, and are in this cafe very ftrong, decreafe very faft, and a lefs number of them is in confequence requifite to complete the charge.

214. Stand-

214. Standing thus infulated, and continuing to hold the charged bottle, a man placed on the ground, touches the hook and the bottom of the bottle, alternately; and thus difcharges it, by a feries of fparks, which prove the ftronger as their number is lefs.

215. I repeat thefe charges and difcharges in the very fame manner as I have done in the preceding paragraphs; only, after having infulated myfelf, I communicate firft with one, then with two, afterwards with three men, likewife infulated; and the charges, as well as difcharges, are completed by ftronger, but fewer fparks, according as the number of infulated men who communicate with either the exterior or interior furface of the bottle is greater; that is, according as the capacity of the bodies that communicate with either of the coatings has been made greater.

216. Thefe experiments may be repeated by making one or more infulated men communicate with the hook, and likewife, one or more infulated men communicate with the bottle; and it may then be obferved that univerfally the ftrength of the fparks increafes, and their number decreafes, in proportion as the capacity of one or both the coatings of the bottle increafes; inafmuch, however, as the electricity of the animated fyftem from which the charge is drawn, will allow it.

217. This *proportionality* leaves no doubt as to the caufe of thofe charges and difcharges by alternation. Let D C (Pl. IV. Fig. 1.) reprefent a plate of glafs; let F A E H B G reprefent the coatings of the plate, which may be called *a* and *b*. It muft be underftood that the coating F A E muft communicate with the Chain; the other coating H B G is meanwhile infulated. As much fire will diffufe itfelf through the coating F A E, to the furface *a* of the plate, as will efcape from the other furface *b* into its own coating H B G; and moreover, as much fire will flow from the Chain into the coating F A E as the capacity of this can contain; that is, the fpark from the Chain to the plate, will be proportioned to the fum of both its coatings. This done, let the plate be parted from the Chain, and the coating H B G, brought to communicate with the ground; and then a fpark will be thrown from the coating H B G, which

will

will be proportioned to the quantity of fire in the ſaid coating H B G, and to the quantity that will be able to get from the other coating F A E into the contiguous ſurface *a*; as ſuch quantity is thereby enabled to drive another quantity equal to it from the oppoſed ſurface *b* into the ground: this ſecond ſpark will be proportioned as we ſaid, to the ſum of the capacities of the two coatings. Proceeding afterwards to make the coatings alternately communicate with the Chain and the floor, the ſame way of reaſoning will ſtand good; except that the faculty in the ſurface *a* of receiving fire, will decreaſe, as it will already have received more of it; and the faculty in the ſurface *b*, of giving fire, will decreaſe as it will already have given more; therefore the ſucceſſive ſparks will be found to decreaſe proportionally; and as, after a given number of ſparks, the ſurface *a* will have received more fire, and the ſurface *b* will have given more, (proportionally to the greater ſum of the capacities of the coatings,) ſo a leſs number of ſparks will now be required to complete the charge.

218. Analogous obſervations may be made on the diſcharge of the plate. Let the plate D C be charged and inſulated. When I touch from the floor the coating F A E, I receive the exceſſive fire accumulated in it, and draw, beſides, from the ſurface *a* as much exceſſive fire as the coating H B G can give to the other ſurface *b*, electrified by deficiency. If I afterwards touch the coating H B G, I give to it as much fire as can adhere to the deficient ſurface *b* (which is equal to the quantity that may diffuſe itſelf from the abounding ſurface *a* into its own coating) and moreover, as much as is neceſſary to fill up the deficiency in the coating H B G; and if the two ſurfaces are ſucceſſively and alternately touched, the ſame effects will obtain, except that the ſparks will ſucceſſively decreaſe as the electricity remaining in the two oppoſed ſurfaces will decreaſe; becauſe when, after a given number of ſparks, I touch the coating F A E, a leſs portion of natural fire muſt diffuſe itſelf from the oppoſed coating H B G into the contiguous ſurface *b*, there to acquire a deficiency of an equal denſity with that of the ſurface *b* already diminiſhed, &c. And as the decreaſe of the deficiency in *b*, and of the exceſs in *a*, is accelerated proportionably to the ſum of the capacities of

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the coatings; ſo the number of the ſparks neceſſary to complete the charge, will diminiſh proportionally to the ſum of the ſaid capacities. It is an agreeable ſpectacle to ſee a bottle ſuſpended to the Chain, when both (the Chain and the bottle) are ſupplied, each with its own electroſcope. While I touch the Chain, the threads of its electroſcope fall down, and thoſe of the electroſcope of the bottle diverge, being become electrified by deficiency: if I touch the bottle, the threads of its electroſcope fall likewiſe, and thoſe of the Chain diverge, having again acquired an electricity by exceſs: the ſparks and divergences thus continually keep decreaſing, and a leſs number of ſparks becomes required to complete the charge, as the capacity of the Chain (with which the interior ſurface of the bottle communicates) and the exterior coating of the bottle are greater: ſo that the ſparks increaſe in force as they diminiſh in point of numbers.

219. Therefore, this ſame proportionality, while it diſcovers the cauſes of the charges and diſcharges by alternation, reſolves a ſeemingly great objection againſt the theory. It is this: if an electricity is produced and deſtroyed by turns, in each of the ſurfaces of the inſulating body, though the other ſurface remains inſulated, it ſeems that ſuch electricity is produced or deſtroyed, though no correſpondent contrary electricity is produced or deſtroyed at the ſame time, in the oppoſed ſurface, as the theory ſhould require. But an analyſis of the fact, grounded on the conſideration of the aforeſaid proportionality, proves that the electricity which is thus produced or deſtroyed by thoſe alternative contacts, is in this caſe alſo governed by the contrary electricity that may be produced or deſtroyed at the ſame time on the oppoſed ſurface, proportionally to the capacity of the coatings. I ſuſpend a bottle to the Chain by its hook; and taking care that it be exactly inſulated, I excite the electricity in the Chain. As ſoon as the two threads annexed to the exterior ſurface begin to diverge, I touch the Chain, and there remains no electricity either in the Chain, or in any part of the bottle. The electric fire of the Chain was accumulated on the interior ſurface of the bottle in as great a quantity as it was able to repel other fire from the exterior ſurface,

ſurface, into the coating and the threads, which were as a part of the bottle; and all theſe were in reality electric by exceſs. Now, when I touch the Chain, I take the exceſſive fire from the Chain and the interior ſurface of the bottle; and correſpondently, the natural fire which was kept out of the exterior ſurface of the glaſs, and harboured in the coating and threads, falls back into it.

220. Moreover, this ſame proportionality ſhews to us that it is not abſolutely neceſſary, in order to diſcharge a bottle, to complete a communication between the two ſurfaces; ſince in touching them alternately, the diſcharge is completed by alternative ſparks; the number of which is leſs, as the capacity of the coatings is greater. Whence it neceſſarily follows that if, while one coating communicates with the ground, the other is alſo brought to have a relatively infinite capacity, the diſcharge will be completed, though the latter coating does not communicate either with the other, or with the ground. In fact, I have many times, when my long deferent threads with which I uſe to try the atmoſpheric electricity, have been animated with a continued, and vehement, though not fulminating electricity. I have, I ſay, often taken a pleaſure to touch the ſaid threads (uſing always the neceſſary caution) with the ſurface of a charged bottle, which ſurface was repleniſhed with an electricity contrary to that of the threads; and in a very ſhort time the bottle was diſcharged, and even charged in a contrary manner. If as many men are inſulated as will equal the capacity of the bottle, and all communicate together; when I preſent to them the hook of a charged bottle, I impart to them the half part of its exceſſive fire, and, in the ſame time, the ground will ſupply the exterior ſurface with the half part of that fire of which it has been deprived. If the capacity of the inſulated men is a thouſand times greater than the capacity of the bottle, only one thouſandth part of the charge will remain in it. It is true that a diſcharge thus made by forming a communication between the oppoſed ſurfaces of the bottle, and two ſeparated ſyſtems, will be leſs violent than if it were effected by forming the communication between the two oppoſed ſurfaces, juſt as a ſpark by exceſs and deficiency together, will be leſs violent than a ſpark produced by an exceſs alone, or by a deficiency alone.

221. Laſtly,

221. Laftly, it follows as a neceffary confequence of all thefe experiments, that each of the contrary electricities that may be introduced on the two oppofed furfaces of an *infulating* body, furpaffes by far any electricity that may be introduced into a *deferent* body, even of a much larger dimenfion :—thus a hundred fparks given to a coating, will neverthelefs introduce into the plate, itfelf, but a fmall charge. Let us add here an experiment which may be called a charge *by dimidiated alternation:* I infulate myfelf between the Chain, and a bottle which communicates with the ground; and I draw fucceffive fparks from the Chain, to which I prefent, at different times, the hook of the bottle: after a hundred fuch fparks the bottle is found to be but imperfectly charged.

222. The fame is ftill better proved by difcharges. Standing upon the ground and holding the bottom of a charged bottle, I alternately prefent the hook of it to the hand of an infulated man, and touch the man, after removing the bottle: after a hundred fuch alternative operations, the bottle has electricity enough left to give me a ftroke.

223. We fhall fee in its proper place, that the great capacity of infulating bodies arifes from the combination of the two electricities that take place on both their furfaces. If from the exceffive thicknefs of thefe bodies, or in general, a want of communication between their oppofed furfaces, fuch combination is prevented, no greater quantity of fire can be accumulated on, or taken from, them, by the mere help of a communication with the Chain, than what could be added to, or taken from, deferent bodies. We fhall fee befides, that the capacity of deferent bodies is no more than that of their furfaces. Let us now proceed to other fimple charges and difcharges, made in different ways.

224. Among them are the charges and difcharges made by the means of *brufhes* of water; which ferve ftill better to confirm the theory. From the conductor C (Pl. IV. Fig. 2.) a rod of brafs is fufpended, which dips in the water of the bafon K I, placed on a ftool of cement M L; I hold by the infulating handle A B the bottle C D, into which one arm of the fiphon P A F, is alfo dipped, and the water iffues from the other arm, and falls into the bafon K I; meanwhile I touch with my finger E the outward coat-

ing of the bottle. As ſoon as the electricity of the conductor is excited, the *bruſh* of water P ſpreads itſelf, and the bottle becomes charged.

225. I repeat the experiment, but without touching the exterior ſurface of the bottle; when I find the bruſh of water is no more; the bottle takes no charge; and when I try it, as I did juſt now, I receive no ſtroke.

226. I repeat once more the experiment, but when the bruſh iſſues I touch and leave the bottle alternately; the conſequence is, that the bruſh does not ſpread itſelf but correſpondently to the time in which I touch the bottle; and the bottle gives no ſtroke but in proportion with the time during which it has been touched.

227. Let now the bottle be ſuppoſed charged, let the baſon lie on the ground, and the communication with the Chain ceaſe; if I let the water iſſue without touching the outſide of the bottle, the bruſh does not ſpread, the bottle takes no charge. If I take away the ſiphon, and ſubſtitute the hook, I receive a ſtroke. If I conſtantly touch the bottle, it looſes its charge and I have no ſtroke. If I touch it and leave it by turns, it is diſcharged proportionally to the time during which I have touched it; and gives a ſtroke which is proportioned to the time during which it has remained untouched.

228. Whence it generally reſults that theſe laws of the bruſh and of the ſtroke, make it viſible and audible, that an electricity cannot be raiſed on a ſurface of an inſulating body, but inaſmuch as a contrary electricity is raiſed, at the ſame time, on the oppoſed ſurface; and that ſuch an electricity cannot be taken away but inaſmuch as the contrary electricity is taken from the other ſurface.

229. Charges and diſcharges by motions, ſucceed more eaſily, and prove the ſame things. I hold the bottom of a bottle, and preſent its hook to a pendulum B (Pl. V. Fig. 4.) of a great electric capacity (it is made with a ſheet of gilded paper, folded in the ſhape of a cylinder) which hangs by a long ſilk thread, near either the Chain, or the Machine D; this pendulum, as it vibrates, carries ſparks from the conductor to the hook, and the bottle becomes charged. I afterwards raiſe the bottle by its hook, and preſent it

to

to the pendulum, and thus make the latter carry to the outſide of the bottle, the fire that had been driven from it by the fire introduced through the hook, into the inſide of it: and thus the bottle is diſcharged.

230. I can likewiſe diſcharge the bottle by continuing to hold it by its bottom, and keeping the pendulum in the middle between my other hand and the hook of the bottle: in this caſe, the pendulum takes the exceſſive fire from the inſide of the bottle, and, through my body, carries it to the exterior ſurface of it; this will equally obtain, whether I am inſulated or not.

231. In charges, as well as diſcharges, the pendulum vibrates at firſt more ſtrongly, becauſe it tranſports more fire in proportion as it has yet tranſported leſs, if a charge is producing; or in proportion as it has more to tranſport, if a diſcharge is effected; and the oſcillations are thus continually decreaſing: with this obſervation, however, that the oſcillations in a diſcharge, as they are produced by poſitive and negative electricities together, are quicker than thoſe of a charge.

232. If it were intended more minutely to examine by the means ofmotions of a *ſimple preſſion*, the manner in which charges and diſcharges are effected, the truth of the theory would appear with ſtill more clearneſs. I. I fix an electroſcope to the exterior coating of a bottle; I ſuſpend this bottle to the Chain ſupplied with its own electroſcope, and I conſtantly touch the bottom of the bottle: when I find, that the electroſcope of the Chain gradually increaſes its divergency, as the charge of the bottle is increaſing; but yet, that the electroſcope of the bottle remains without motion. The reaſon is this;—in proportion as the exceſſive fire accumulates itſelf in a greater quantity in the inſide of the bottle, and has driven a greater quantity from the outſide, it meets with a continually greater reſiſtance from accumulating itſelf farther, and driving away more fire from the outſide; conſequently, it is kept back, and is accumulated in a greater quantity in the Chain. But as, at the ſame time, all the fire that is driven from the outſide of the bottle, can flow through my body into the ground, it conſequently cannot produce any exceſs in the exterior coating: nor, for the ſame reaſon, can any part of the deficiency produced and

maintained in the exterior ſurface of the glaſs, ſtop and manifeſt itſelf in the ſaid exterior coating.

233. II. I repeat the experiment with only this difference, that I now and then take my hand from the bottle; and I obſerve, at every time, that the electroſcope of the Chain immediately attains its greateſt divergency; and alſo, that the electroſcope of the outſide of the bottle, acquires divergence proportioned to the charge already produced, inverſely. That is to ſay, as the outſide of the bottle becomes inſulated, no fire can eſcape from it; no fire, therefore, can flow to the interior ſurface: conſequently, it is forced to remain and accumulate itſelf in the Chain, and the electroſcope immediately diverges to the utmoſt. Moreover, in proportion as the charge will be advanced, a leſs quantity of fire will be driven from the exterior ſurface of the glaſs into both its coating and the electroſcope annexed to it; ſo that a leſs divergence will conſequently take place in the latter; but, when at laſt the charge is completed, no divergence will be perceived, though I take my hand from the bottle.

234. If, after completing the charge, we paſs to the diſcharge of the bottle, ſuſpended for inſtance to the Chain, we ſhall ſee that when the diſcharge is effected with a conducting bow which is not inſulated, no divergence will remain either in the electroſcope of the Chain, or in that of the bottle; unleſs perhaps ſome ariſes afterwards from a remainder of the *overflowing* electricity, as I will mention hereafter. But then if I effect the diſcharge with an inſulated bow, ſome divergence will remain in the electroſcope of the Chain, and an equal one will ariſe in the electroſcope of the bottle. The reaſon is, that when the contrary electricities of the two ſurfaces of the bottle will have deſtroyed and compenſated each other, the exceſſive electricity of the Chain and hook will diſtribute itſelf to an equal denſity into the inſulated conducting bow, and the two coatings of the bottle. But, in the charge of *independency*, which I ſhall deſcribe in the paragraph 240, the conducting bow will leave no remainder of electricity; and the reaſon will be that, in that caſe, no fire will be accumulated on one ſurface of the bottle and its coating, but what will have been entirely extracted from the oppoſed ſurface and coating.

235. We

235. We might now difcufs the cafe of a difcharge of the bottle, by alternation, but we have already confidered it. To be fhort, when a coating is touched, the electrofcope annexed to it falls, and that annexed to the other furface rifes, though, we muft obferve, in proportion to the remainder of the charge.

236. In order to examine the charges and difcharges produced by fuch motions as refult from alternate attractions and repulfions, I ufually prefent to the coating of the bottle, a fmall piece of tinfoil †, hanging by a thin filk thread. And firft, if, as in the parag. 232. I prefent fuch pendulum of tinfoil to the exterior coating, it receives no motion; becaufe, as we have feen, there is no electricity in the coating, fince it communicates with the ground; but if I prefent the fame to the Chain, which is, in a manner, a prolongation of the interior coating, then, if the filk thread is fomewhat damp, the tinfoil is immediately attracted; or, if it be very dry, I only need touch it with my finger *, and the tinfoil flies to the Chain; from which likewife receiving an electricity by excefs, it is inftantly repelled. In this ftate of things, if I prefent that fame piece of tinfoil to the exterior coating, it is attracted by it, as it would be by any other part of the ground: fo that it diffufes into the coating, or into the floor with which the coating communicates through my body, all the excefs it has received from the Chain, or rather from the interior coating. If I prefent it again to the Chain, and if it be very dry, it will not be either attracted or repelled, till it has been touched.

237. II, in the cafe of the No. 233. the piece of tinfoil, when repelled by the Chain, is alfo repelled by the exterior coating; which fhows that this exterior coating, really becomes electric by excefs in confequence of the natural fire of the outfide of the glafs which ftops in it, while I ceafe to touch it; though (which

* Befides the general law, that bodies fimilarly electrified mutually repel each other, and differently electrified bodies mutually attract each other, I muft here take it for granted (which I fhall hereafter prove) that a body not electrified cannot be moved by one that is electrified, but inafmuch as the latter will be able by the means of its electric atmofphere to introduce into the former an electricity contrary to its own: and, in order that the Chain may introduce a contrary electricity into the piece of tinfoil abovementioned, it muft be enabled to drive away the natural fire in it, either through the damp thread annexed to it, or through any other body in contact with it.

† Or German leaf-brafs.

is extremely conformable to all that has been ſaid) this laſt repulſion produced by the coating, is leſs ſtrong in proportion as a greater quantity of charge has been introduced, while I ceaſed to touch it, as has been ſaid before.

238. In the caſe of the No. 234, that is, in the caſe of a complete diſcharge, there is nothing farther to be obſerved with regard to the piece of tinfoil, except perhaps that when it was in a ſtate of attraction or repulſion, it would have been reduced to its natural ſtate, if a communication had been formed between the two oppoſed ſurfaces of the glaſs, by the means of a deferent body communicating with the ground; or the ſame piece of tinfoil would have been brought into a ſtate of repulſion proportioned to the quantity of electricity remaining in the two coatings, (234) if the communication had been only formed with an inſulated body.

239. We are now to examine the caſe of a bottle that is charged, and has its two ſurfaces exactly inſulated. Let us ſuppoſe, firſt, the caſe of a bottle ſuſpended to the Chain. I. If the piece of tinfoil be preſented, firſt of all, to the bottle, and even is made to touch it, yet it will not move, if the outward coating of the bottle has been brought to have no electricity in it. II. But if the ſame be preſented, firſt, to the Chain, I ſhall no ſooner have touched it, but it will be drawn, and then repelled; becauſe it will find in the Chain an exceſs correſpondent to that in the inſide of the bottle. III. If the piece of tinfoil be immediately preſented to the exterior coating it will then be drawn and repelled by it; becauſe it will have received an exceſs from the Chain, and will now find a correſponding deficiency in the exterior coating. The reaſon is this: the Chain, which is a prolongation of the interior coating, required to have in it a certain exceſs, in order to be enabled to maintain, jointly with the exceſs on the inward ſurface of the glaſs, the deficiency in the exterior ſurface of it; ſo that the exterior coating then retained its natural quantity of fire; now, in conſequence of the chain having drawn the piece of tinfoil, ſome portion of its exceſs has been taken away, it will of courſe be no longer able to maintain in the outward ſurface of the glaſs, as great a deficiency as it did at firſt; ſome part of the natural fire in the outward coating will then paſs into the outward ſurface of the

the glafs under it; and confequently that coating will contract an electricity by deficiency. IV. Laftly, if, the bottle remaining exactly infulated, as above, I make the piece of tinfoil vibrate between my finger and one of the coatings, the pendulum will conftantly draw from that coating, a quantity of electricity equal to fuch contrary electricity as may flow from the oppofed furface into its own coating: then the pendulum will ceafe to vibrate, and muft be prefented to the other furface, where it will begin again to play, and at laft likewife ceafe; and in fhort a difcharge will be obtained by ofcillations that alternately ceafe and begin again: they are reproduced by turns, in the fame manner as electric figns are reproduced by turns between the Machine and the ground; or between the ground and the Chain, when both fyftems are infulated.

240. Every body evidently fees how completely conformable thefe motions are to the theory, and I may eafily exempt myfelf from giving any farther explanation: I fhall therefore conclude this chapter with a more explicit account of a fine experiment of Dr. Franklin, which I have mentioned before; I call it a charge of *independency*. I hold by its bottom B the bottle A B annexed to the hook of the conductor C. (Pl. III. Fig. 10.) The globe or cylinder *a b c d*, is rubbed by a cufhion infulated by the fupport of glafs V, which is fixed in a hole of the beam Q (Pl. I. Fig. 1.) then I find the bottle does not become charged; nor is the globe able to carry into it any other fire than that contained in the cufhion itfelf. But if I join the outfide of the bottle to the cufhion, by the means of a wire of metal B R, then, notwithftanding that I do not touch the bottle, it becomes charged, and even more ftrongly than ufual: the reafon is, becaufe the globe is fupplied with the fire which it accumulates in the inward furface of the glafs from the very outward furface of it; and the concurrence, in this cafe, of the extracting with the accumulating force, increafes the intenfity of the charge. As Charges are ufually effected by making one of the furfaces communicate with the ground, in this cafe one of the two forces is, as it were, loft in the infinite capacity of it, and Charges confequently only correfpond to the other force.

241. Certainly, I do not think that any other experiment can ſo well, and ſo ſimply at the ſame time, demonſtrate both the equality and contrariety of the two electricities, as does that charge of independency; eſpecially if ſuch charge be compared with the diſcharge that I call likewiſe of *independency*; which is effected by a Man ſtanding inſulated, or which is ſtill better, by the means of an inſulated bow.

CHAP. III.

In which the theory is confirmed by a combination of two or more charges and diſcharges.

242. I Hold with one of my hands the bottom of the bottle A B, and with the other, the bottom of the bottle *a b* (Pl. II. Fig. 6.) and I keep their hooks in contact with the Chain: in which caſe, the electroſcope annexed to the one of them, attains but ſlowly its greateſt divergence. When it has attained it, I remove the two bottles at once, and place them on an inſulated bench; then I immediately take again the bottle A B, and, with my other hand, touch the hook of it; I do the ſame with the bottle *a b*; each of them gives me a ſtroke proportioned (every other circumſtance being equal) to its own capacity. This is becauſe the electric fire of the Chain tends equally to accumulate itſelf in all the equal parts of the coated inward ſurfaces, and to drive correſponding quantities of natural fire from the other outward ſurfaces.

243. I charge again the two bottles jointly, and in the ſame manner; and after removing them from the Chain, I bring their hooks into a mutual contact: but no light is produced, no ſpark flies, though the capacities of the two bottles be ever ſo different: this is becauſe the ſame ſtream of fire being diſtributed into the two bottles, proportionally to the capacities, retains in them an equal denſity; conſequently, the two exceſſes reciprocally and equally repel each other.

244. I ſay that the two charges jointly of the two bottles are proportioned to their two capacities together; but this is when

6 other

other circumſtances are equal. I make uſe of two bottles of the ſame ſize, and the coatings of which are equal; only the bottle *a b* is thicker than the bottle A B: and the latter (the thinneſt) ſtrikes me more violently than the other. The reaſon is this; the exceſſive fire that flows into the bottle *a b* finds more difficulty to drive through its ſubſtance the natural fire from the oppoſed ſurface: now, as the exceſſive fire cannot accumulate itſelf on the interior ſurface of the bottle, but in proportion to the quantity of natural fire which it can drive from the outſide of it, it follows that the intenſity of the charge muſt be leſs when the glaſs is thicker.

245. I have hitherto conſidered the caſes of diſcharges effected ſubſequently to the charges; now I proceed to examine the caſes of charges and diſcharges effected at once. Having jointly charged the two bottles A B, *a b*, I place them on an inſulated ſtool, then I take them both in my hands, by their long uncoated necks, and incline them ſo as to make the hook *a* touch the bottom of the bottle B; and in the inſtant the communication is effected, a middling ſpark is thrown out. Then I bring the hook A into contact with the bottom of the bottle *b*, and at that inſtant the diſcharge is completed. That is to ſay, when at firſt the bottle B touches the hook *a*, a quantity of exceſſive fire paſſes from *a* into B, equal to that which it can drive from A into the air, or ſuch bodies as ſtand at a little diſtance, for inſtance, my hands, and to the natural fire that may be drawn into *b*, from the air, or bodies placed near, by the joint efficacy of the deficiency in B, and exceſs in *a*. But when afterwards the hook A and the bottle *b* are likewiſe brought into contact, all the exceſs in A throws itſelf out to fill up the deficiency in *b*; becauſe reciprocally all the exceſs in *a* can then run to fill up the deficiency in B: ſo that, examining afterwards the two bottles I receive no ſtroke (except perhaps from ſome remain of charge, of which I ſhall ſpeak in its place) if the bottles were of an equal capacity.

246. The reaſon is this, if the capacities of the bottles are unequal, ſuch bottles jointly and equally charged, and then made to communicate together as before (the hook of the one with the bottom of the other) are found, after ſuch communication, to

have contrary electricities on their homologous furfaces; and thefe electricities are proportioned to the difference of their capacities. Let, for inftance, the capacity of the bottle A B, be to the capacity of the bottle *a b*, as 20 to 5: if I make the hook of A communicate with the bottom of *a b*, and likewife the hook of *a*, with the bottom of A B, $\frac{5}{20}$ of the excefs in A will pafs into *b*, and fill up its whole deficiency; and the whole excefs in *a* will pafs into B, and fill $\frac{5}{20}$ of its deficiency. In confequence of this difcharge of the fourth part of the excefs in the bottle A B, the other *a b*, will be brought to have the natural quantity of fire, on both its oppofite furfaces; that is to fay, will be thoroughly difcharged. But fifteen parts of excefs, we muft obferve, will remain in A, and fifteen parts of deficiency in B: thefe remaining parts will alfo diftribute themfelves into the two bottles, in proportion to their capacities; that is to fay, $\frac{3}{15}$ of the excefs in A will pafs into *b*, and $\frac{12}{15}$ remain in A; and a quantity of natural fire will alfo from *a*, pafs into B, and fupply $\frac{3}{15}$ parts of its deficiency, fo that only $\frac{12}{15}$ will remain. All this may be proved in different ways. I. By feverally difcharging the two bottles: both the light and crack of the fpark, as well as the ftrength of the fhock, will be found to anfwer to their refpective capacities. II. By making the two bottles again communicate, the hook of the one with the hook of the other, and the bottom of the one with that of the other: a fecond difcharge will in this cafe be obtained, which will alfo be found to be proportioned to the difference of the capacities of the bottles. I have, feveral times, amufed myfelf to charge, jointly, a great jar of cryftal, which contains within the capacity of its lining, fixteen pints of water, and a fmall lined jar which only contains the fourth part of a pint: then, holding the latter with an infulating handle, I touched at once, with its hook and bottom, the bottom and hook of the large jar, then the hook and bottom, and fo on, a number of times, alternately; when I had no fmall pleafure to obferve how the fparks from the alternate difcharges went gradually decreafing; and how in each of thefe decreafing difcharges, two fhort threads annexed to the fmall jar, at once fell, and then gradually refumed a lefs divergence than before; while two threads annexed

annexed to the larger jar, without falling, only loſt a little of their former divergence.

247. Now, theſe diſcharges thus jointly effected a number of times, by alternately bringing together the contrary ſurfaces of two charged inſulating bodies, are thoſe diſcharges which I call of *ſimple diſtribution.* Let us now ſubjoin an experiment which certainly will confirm the theory laid down in this work with regard to the cauſe of charges, and alſo ſerve to illuſtrate the manner in which complicated charges and diſcharges, which are the ſubject of this chapter, are effected. I charge a bottle A B, from the Chain, and holding it by its bottom, I preſent its hook to the hook of a ſimilar bottle *a b*, which I hold in my other hand. At that inſtant, a ſpark flies between the two hooks, and I receive a ſhock: half part of the exceſs in the charged bottle A B (I ſuppoſe the two bottles to be of equal capacities) runs into the bottle *a b*, which is without charge; and an equal quantity of natural fire runs from the outward bottom of the latter, through my own body, to ſupply half part of the deficiency in the outward bottom of A B. In fact, if I afterwards try ſeparately to diſcharge thoſe two bottles in the uſual way, I receive two different ſhocks, which I find to be equal to the former one, and, of courſe, equal to each other.

248. It is a difficult matter to find thus two bottles of equal electric capacities. I try them by charging them, jointly: if their electricities exactly deſtroy each other, when I make their contrary ſurfaces communicate together, I call them *equivalent bottles.*

249. I take a ſeries of ſuch *equivalent* bottles, A, B, C, D; I charge the bottle A, and graſping in my hand the bottle B, which is without charge, I bring their hooks together: half part of the charge in A diſtributes itſelf into the bottle B. I then lay by the bottle A; take up the bottle C, and make its hook touch the hook of the bottle B; when the half of the charge juſt introduced in B, diſtributes itſelf into C. I lay by the bottle B, take up D, and make its hook communicate with that of the bottle C; when again, the half of the charge introduced in C, diſtributes itſelf into D. I then reſume each of the four bottles ſucceſſively, and diſcharge them in the uſual way, through my

own ſubſtance; when, by eſtimating as far as I am able, the intenſity of the charge, from the ſtrength of the ſpark and ſtroke, I find I muſt ſay, that the charge remaining in A, is double to that remaining in B; that remaining in B, double to that remaining in C; and that in C, equal to that in D, the charge of which has not been farther divided.

250. But let us return to charges made jointly; and ſince we have examined *ſimilar joint* charges, that is, thoſe by which ſimilar electricities are raiſed on the homologous ſurfaces of the two bottles; now let us conſider the contrary charges, that is, thoſe by which contrary electricities are raiſed on the homologous ſurfaces, by applying them either to the ſame, or to different ſyſtems. I raiſe the bottle *a b*, by its hook, and keep its bottom in contact with the chain, while, with my other hand, I hold the bottom of the bottle A B, and touch the chain with the hook annexed to that bottle. This done, I lay the two charged bottles on the inſulating ſtool (Pl. II. Fig. 6); I then take them by their necks, and inclining their hooks, I make the hook of the one communicate with the bottom of the other, when I find that no ſpark appears. Having again raiſed the bottles, I touch hook with hook, bottom with bottom, and they are diſcharged: beſides, if while I approach the hooks to each other, I hold the bottles by their bottoms, I receive a vehement ſtroke. The reaſon is this, all the exceſs raiſed on the outſide of the bottle *a b*, flies through my body, to fill the deficiency raiſed on the outſide of the bottle A B: mean while all the exceſs raiſed within the bottle A B, throws itſelf into the hook of the bottle *a b*, and fills the deficiency likewiſe raiſed on its inſide.

251. Let the Machine and Chain be inſulated; I hold the bottle *a b*, and preſent its hook to the Machine, while, with my other hand, I preſent the hook of the other bottle to the Chain. Then I remove them at once, and preſent the hooks to each other; when I receive a ſtroke, and the bottles are diſcharged. This is to be explained thus: the Machine has extracted the natural fire from the inſide of *a b*; and, through the Chain, has introduced it into the inſide of A B: therefore the natural fire has been driven from the

out-

outſide of A B, and a quantity of exceſſive fire has been accumulated on the outſide of *a b*.

252. We are now to treat of the *conſecutive joint charges*. I ſuſpend a bottle to the Chain; it being thus inſulated, takes no charge. I touch its bottom with the hook of another bottle, of which I graſp the bottom; both become charged; and, if I try them, give me a ſtroke. If I touch the hooks with each other, though I alſo touch the bottom of the bottle, there is not the leaſt beginning of a charge. If I graſp the bottom of *a b*, (Pl. II. Fig. 6.) and raiſe A B by its hook, ſo that its bottom be touched by the hook of *a b*, both become charged. The exceſſive fire which accumulates itſelf from the Chain, on the inſide of the bottle annexed to it, drives an equal quantity of natural fire from the outſide of the ſame bottle, into the inſide of the other; and this fire, in its turn, drives from the outſide of the latter bottle through my body, an equal quantity of natural fire.

253. This experiment may be made with three, four, or more bottles that ſucceſſively hang from one another by their hooks: but as the number of theſe bottles increaſe, the intenſity of the charges will decreaſe: the reaſon is, becauſe the total thickneſs of the glaſs, proportionally to which the charges muſt decreaſe, will then be increaſed.

254. It ſometimes happens that when I ſucceſſively diſcharge the ſaid bottles, one of thoſe in the middle will give a greater ſtroke than that before it; this ariſes from two cauſes chiefly, viz. from the more exact inſulation of one of the ſucceſſive bottles; and from the leſs exact diſcharge of that which is before it. If any portion of the exceſs that is accumulating in the inſide of the preceding bottle, flows from the inward into the outward coating of it, by means of any communication ariſing from dampneſs or moiſture, no other charge will take place within ſuch a bottle, but what may ariſe from ſuch portion of exceſs as cannot eſcape through the ſaid different particles: and in the meanwhile, if the following bottle has both its ſurfaces better ſeparated from each other, in conſequence of a more exact inſulation, the charge it will receive will be equal to the two portions that will contribute to form it; that is, both to the portion of exceſs that has thus

thus externally efcaped, and to fuch portion of fire as remained accumulated within this fame preceding bottle.

255. Moreover, the charges of the two fucceffive bottles may be equal to each other, and yet the difcharges appear very unequal. If, for inftance, the hook of the preceding bottle has any part of it too rough, or too pointed, the difcharge will be effected in a divided manner, and confequently give a lefs ftroke. On the other hand, if the hook of the fucceffive bottle has in it any portion of moifture, of which the hook of the preceding one we may fuppofe was free ; this will open a wider way to the difcharge of fuch bottle, as I will fhew in its proper place : whence it will happen, that the fpark leaping more inftantaneoufly, will ftrike with a greater force. I confefs that the reafon I give here of fuch unequal difcharges arifing from equal charges, is not of itfelf very obvious ; but this is becaufe the inftant by which one of the difcharges is, as I fay, made longer, and confequently weaker, than the others, is fo fhort, that our fenfes cannot perceive it, nor find the difference it produces between the times in which the above mentioned difcharges are feverally performed.

256. I fhall obferve here, by the by, I. That *fucceffive* charges of bottles may be effected in fuch manner as to have the homologous electricities takeplace on *contrary* furfaces of them. If I hold, for inftance, a bottle by its hook, and bring its bottom to touch the bottom of another that hangs from the chain by its hook, the latter will be changed by excefs in its infide, while the former is fo on its outfide, &c. II. Any charges whatfoever made by the means of the Chain, fucceed as well, though in a contrary way, as thofe made by the means of the Machine: that is, there refults no other difference befides that of the electricities produced upon the fame furfaces, being contrary. III. All the experiments which I have defcribed as made with bottles, will equally fuceeed with glaffes of any other fhapes ; fuch as common veffels, plates, tubes, &c. provided they are thin enough, properly coated, and made ufe of confiftently with their form. All thefe obfervations are too obvious to be more explicitly infifted upon :—I fhall only obferve, that the general mutual agreement between all the above experiments, throws a moft complete light on the fubject, and makes the certainty of the theory perfectly evident.

257. How-

257. However, in ſome caſes, a certain ſhape in the glaſs is more convenient than another. For inſtance, when I examine how far the charges are leſſened by the thickneſs of the glaſs, I prefer plates of glaſs to bottles, becauſe the thickneſs of the former is more uniform, the difference between them is more eaſily eſtimated, and their electric *capacities* may be made more completely equal to one another: they will not, it is true, be ſo eaſily managed, but the exactneſs will be greater, and the obſervations that will reſult from the experiment will prove more ſimple and eaſy. Thus, I ſelect two plates *ABCD*, *abcd* (Pl. VII. fig. 10.) the one is thicker than the other in a given proportion, but their metallic coatings are completely equal. I place one of the plates on one of my hands, and the other plate on my other hand; I then preſent them to two rods of braſs that proceed from the Chain; and, when the electroſcope informs me that the charges are completed, I remove both plates in one and the ſame inſtant (which is eaſy, from the uſe we have of making ſimilar motions with both hands at once); this done, another perſon immediately takes both plates by their angles, and keeps them in an horizontal ſituation, at a proper diſtance from each other; then I take with handles of ſealing-wax *I*, *i*. the two conducting bows, which are bent, the one in the ſhape ⁀‿, the other in the ſhape ‿⁀, and I place them ſo, that each communicates with the different ſurface of each of the two plates; when I find the effect to be that the exceſs in E, and the deficiency in H, and correſpondently, the deficiency in *h*, and the exceſs in *e* deſtroy each other, as far as they are equal to each other. Beſides, the overplus of exceſs in E diſtributes itſelf; that portion which remains in E, is to that portion which paſſes into H, as the thickneſs of ABCD, is to the thickneſs of *abcd*: and the overplus of the deficiency in *h* is communicated to *e* in the ſame proportion: ſo that I find the electricities in ABCD, inverted, and they are to the electricities that remain in *abcd*, as the thickneſs of *abcd*, to the thickneſs of ABCD.

CHAP.

CHAP. IV.

In which the Theory laid down in this Work is confirmed, by charges and discharges of glasses, to which the vacuum *serves as a coating.*

258. IF an insulating body, though capable of itself of taking a charge, be placed in a space exactly emptied of air, it cannot be charged: the reason is, that any quantity of fire that will flow to the one of the surfaces of such body, will, in consequence of the exact vacuum, diffuse itself to the other surface; therefore, there will be no excess in the one, no deficiency in the other.

259. But as the vacuum in the air-pump never is brought to perfection, then it follows that the insulating body placed in such vacuum, will receive some beginning of a charge; which will be proportioned to the imperfection of the said vacuum; this can be perceived in that experiment which I formerly mentioned, in the parag. 272, of the *Artificial Electricity.* Let the fig. 13. of Plate IV. represent the necessary apparatus, which consists of a glass bell emptied of air, with the half of a ball, or an hemisphere, of very thin glass, inverted on the bason. The electric ray BC flows from the Chain, through the rod AB, into the top of the said hemisphere: the ray seems, at first, to stop in C; and, correspondent to it, a faint light appears on the top of the pipe of metal which rises from the bason of the air-pump; but soon after, the electric ray diffuses itself from C all over the surface of the hemisphere, and runs along it (in *D*) to the bason under it: nor does this ray CD, constantly keep to the same track, but keeps turning about on the surface of the hemisphere of glass. Now, both this stopping of the ray in *C*, and its turning afterwards around the hemisphere while flowing along it, show that a small portion of excess is accumulated on the surface of the said hemisphere, both on that place to which the ray flows at first, and on those about which it winds. The light on the top of the pipe of metal, indicates that a small portion of fire has been driven from the inward surface

furface of the hemifphere, and in fhort, that there is a beginning of a charge, at leaft of a tranfitory one, proportioned to the imperfect *deference* of the vacuum. In fact, according as I let in any additional portion of air, the ray BC, not only becomes more vifible, lefs continuous, and narrower; but it ftops longer in C; leaps in a greater quantity to fuch other points of the hemifphere as are fituated obliquely to the rod AB; fubdivides itfelf into a greater number of rays as it flows along the furface of the hemifphere; and growing gradually fainter, fucceffively vanifhes. Correfpondently to this, more vivid rays of light throw themfelves, from the inward furface of the hemifphere, into the top of the pipe of metal, and even into the bafon, where the latter is in contact with the interior edge of the hemifphere.

260. A fimilar explanation may be given of thofe charges which are raifed on a glafs bell, within which the air has been dilated. The fire which from the blunted rod AB (Pl. IV. fig. 14.) directs itfelf at firft to the bafon, bends afterwards its courfe towards the finger prefented by the hand C, becaufe as the diftance of it is lefs than that of the bafon, the dilated air interpofed between it and the point B, oppofes lefs refiftance to the fire, than the air equally dilated which lies between the faid point B and the bafon. If the whole palm of the hand be laterally applied to the glafs bell, a large quantity of fire will run to the correfponding part of the inward furface of the latter; which fire will drive an equal quantity from the outward furface contiguous to the hand: and thus a fmall charge may be obtained, which would give a ftroke to a man who, keeping his hand *C* applied to the bell, would carry his other hand to the hook A, or to the Chain with which the fame communicates. I fay, a fmall charge, becaufe the fire that will run to that part of the inward furface which correfponds to the hand, will ftop there only in proportion with the very fmall refiftance which the dilated air will oppofe againft its reaching the bafon. If the air-pump be infulated, the charge can then be greater, becaufe when the pump will be *faturatea*, all the fire will be determined towards the hand. Care however muft be taken that the glafs bell be exactly polifhed and dry,

 and

and of a glafs fufficiently thin; that the extremity B of the rod be not too far from the place to which the hand is applied; and that the faid hand be applied to a place fufficiently diftant from both the metallic armature of the neck of the glafs bell, and the bafon on which the fame is placed.

261. An eafier and ftronger charge will be obtained by applying the animated fyftem, not to the infide of the glafs, to which the vacuum correfponds, but to the outfide, which is furrounded by the air of the atmofphere. After the example of the Abbé Nollet, I fixed with cement the neck B of a bottle of thin glafs, within the large neck E of a glafs bell (Pl. III. fig. 9.), fo that the bottom of the bottle remained within the capacity of the glafs bell. Having formed the vacuum, I made the hook A communicate with the Chain, and the bottle became very ftrongly charged: fo that, a perfon who touched with one hand the bafon F G of the air pump, and carried the other hand to the hook, received as ftrong a fhock as could be effected with any bottle of the fame capacity. The Abbé alledged that experiment to prove that the electrical fire can, as it were, ooze through glafs, and efcape through its fubftance. He pretended that while the bottle was charging, the rays of fire never ceafed to flow from the infide of the fame to the bafon, acrofs the dilated air; but in the third volume of his letters, he has been candid enough to give up this opinion. The fact, therefore, is, as I wrote in the letter added to my book of *Artificial Electricity**, that in charging the bottle, a kind of lucid femicircles are feen flying about the bottom of it, which unite together near the loweft part, into one ray that directly proceeds to the bafon of the air-pump. But thefe femicircles and ray, after fome windings, grow languid, and do not appear but in an interrupted manner, and in thofe inftants efpecially when fome more ftrength being ufed in the friction, the electricity of the glafs is increafed: when the friction is made uniformly, they difappear intirely. Care muft be taken, therefore, to keep the friction uniform; and above all, that, while the

* A former work of the author's.

charge-

charge is forming, the electricity of the Chain be not diminished either by the vicinity of a strange body, or any other cause. The reason is, that should any such thing happen, the fire accumulated within the bottle would flow back into the Chain, and an equal quantity would run through the vacuum of the glass-bell, to the bottom of the bottle; so that the charge, instead of moving forwards, would move backwards, and the rays which would then be mistaken for a continuation of the same charge, would, in fact, be the beginning of a new one. The said Abbé used, in this experiment, to cover all the bason of the air-pump with a very thick layer of sealing-wax, which, as every body sees, must stop, in consequence of its repressing quality and thickness, the electric fire which sprung out of the bottle; so that, there is no wonder that, not being able to proceed farther, this fire continued to vibrate and produce a light.

262. The light from the discharge of the above bottle, is indeed very different from the light produced by its charge. The light from the former, at the instant it is effected, forms around the bottle a kind of splendid coat, which, if attentively considered, seems to be composed of small rays, converging to the surface of the bottle. Indeed the different forms assumed by the electric fire, in its springing out of the surface of the bottle, and in its getting back into it, might alone afford proofs of the difference of its directions, in these two cases.

263. That discharge which is made by touching at once the bason FG, and the hook A, is extremely simple; but if, instead of effecting it in that manner, the palm of the hand be applied to the glass bell itself, nearer to the bottom of the bottle, the discharge will then be complicated with a transitory charge of the glass bell. This charge will arise from a great portion of fire flying from the inward surface of the glass bell, to the deficient bottom of the bottle. That part from which the fire will fly, will be the same which corresponds to the hand, as the latter can supply an equal excess on the outside of the bell, and meanwhile the other hand diffuses away the excess accumulated in the inside of the bottle. The light which, in that kind of discharge, abundantly sparkles within the glass bell, between the bottle and

and the place where the hand lies, is a ſign clear enough of a tranſitory charge of the glaſs bell; and the ſtrong ſhocks I have had from it, have moreover thoroughly convinced me of the truth of the obſervation. But ſuch charge never can be, as I ſay, but a tranſitory one, becauſe both the deficiency introduced in the inward ſurface of the glaſs bell, and the exceſs introduced on its outward ſurface, can be diffuſed into the ground; the former through the dilated air, the latter through my body.

264. As the diſcharge of the bell may alſo be obtained by ſimply touching the baſon, it is not neceſſarily *complicated*; therefore, the charges and diſcharges made by the means of a vacuum which I have hitherto explained, may be called both *ſimple* and *ſingle* ones; but there are other diſcharges alſo made with a vacuum, that are *double* or even *treble*, and in which from one charge or diſcharge, two or three others follow. I ſhall rank firſt among theſe ſucceſſive charges and diſcharges, the *double* ones effected by the Abbé Nollet: the apparatus may be ſeen repreſented in Plate IV. fig. 10 and 11. It conſiſts of a ſmall bottle AB with a long neck, which is emptied of air and hermetically ſealed: the neck, which in the figures is repreſented as almoſt naked, muſt be underſtood to be almoſt intirely inſerted into a little tube of tin. As ſoon as this tube is joined to the Chain, copious rays are ſeen throwing themſelves from the inſide of the neck of the bottle thus coated, towards the bottom of it. If a hand E be applied to the outſide, in B, the rays then fly towards it, and follow each other more vivid and continued than before. If the bottom be outwardly coated with a ſheet of tin up to AD, where the neck begins (Fig. 11.) the rays of electric fire appear ſtill more vivid in the naked part of the neck; and whether the naked bottle (Fig. 10.) be held in the hand, or the coated one (Fig. 11.), a ſpark flies out when the other hand touches the little tube FG; but the ſtroke given by ſuch ſpark is more violent when drawn from the coated bottle; and the light with which the neck of the bottle is then repleniſhed is alſo more vivid and copious.

265. I think that it will be ſufficient to ſubjoin here the explanation of thoſe *conſecutive* charges and diſcharges, as I gave it in my letter to the ſaid Abbé, in the parag. 486, 510, of my

my *Artificial Electricity.* I. The exceffive fire of the Chain flows through the fubftance of the little tube, to the outfide of the neck of the bottle which is within it. II. This exceffive fire drives from thence an equal quantity of natural fire: the firft and immediate charge is therefore produced. III. That natural fire, thus driven from the inward furface of the neck of the bottle, accumulates itfelf on the bottom, correfpondently to the hand, or any other more ample body that touches it outwardly; and IV. drives into that body an equal quantity of fire from the bottom of the bottle: this is the fecond charge; I call it, *of confequence*. The explanation of the whole is, in fhort, that the neck, which is coated with the little tube, and the bottom which is furrounded by my hand, are like two different bottles, or plates of glafs, mutually feparated by the portion of the neck that is left uncoated; and they inwardly communicate together by the means of the vacuum. Let us now proceed to the difcharge:—if, while the electricity is excited, the perfon who experiments, touches the coated bottom, or the fame being uncoated, grafps with his hand the whole capacity of it, and then carries his other hand to the little tube, then the two difcharges will be effected at once with a fingle ftroke: that is to fay, the excefs accumulated on the outfide of the neck of the bottle, flies through the body of the man who makes the experiment, and fupplies the deficiency raifed on the outward bottom of the fame; and the excefs accumulated on the inward bottom, runs, through the vacuum, into the inward furface of the neck; and it is that quantity of fire that makes, at that inftant, the neck of the bottle appear as if full of light. With this experiment alfo the Abbé pretended to prove that the electric fire paffes through the fubftance of the glafs.

266. The fame explanation may be given of the fine experiment commonly attributed to the celebrated Mr. Canton, which is made in the vacuum of a barometer. This is the manner in which I execute it. I fill with mercury the incurvated tube ABC (Pl. VI. fig. 2.) forty inches high, and I ufe all the neceffary precautions to obtain an exact vacuum: the arm CB is fhort, fo that, when I have erected again the tube (which I do very flowly, for fear of making the mercury ofcillate), it remains full; and I apply to the opening of it C, a piece of melted fealing-wax.

I then

I then make the top of the barometer touch the Chain, while I grafp with both my hands the body of it, where it is full of mercury. The light that flows within the vacuum AB, affords a moft agreeable fpectacle: but when I have removed the barometer from the Chain, the interrupted flafhes of light, which continue for feveral minutes to fparkle, and fucceffively grow more languid and feparated from each other, afford a ftill more admirable fight. Now every body fees that when the barometer communicates by its top with the Chain (I ufe to coat that top with tin, and hang it by a hook to the Chain) the exceffive fire accumulates itfelf on the furface of it as far as it can fpread itfelf, and drives an equal quantity from the inward furface; which fire goes firft through the vacuum, and thence through the deferent mercury, to accumulate itfelf on that part of the infide of the tube which correfponds to my hands, where it can in its turn diflodge an equal quantity from the outward furface of the glafs; and thus, are the two confecutive charges completed. When I afterwards remove the barometer thus doubly charged, the two charges tend then to deftroy each other: the fire accumulated in the infide correfpondently to my hands, ftrives to diffufe itfelf through the mercury and the vacuum, into the fuperior inward furface, from which it has been at firft expelled; it, therefore, endeavours to expel the fire outwardly accumulated in that place; and, in the meanwhile, it draws the natural fire to that outward part of the tube by which I hold it. While fuch an equilibrium is gradually reftoring, the tranfient fire lightens through the vacuum. In fact, it fuffices that I touch with one hand the top of the barometer, and with the other, the fides of it, to make the fire after it has ceafed to lighten, fparkle again.

267. The experiment which I defcribed in the parag. 346, of my *Artificial Electricity*, is more complicated, and furely is not lefs agreeable, " A thin bottle of glafs, I faid, accurately emptied " of air and hermetically fealed, as foon as it is immerfed in the " electric atmofphere of the Chain, appears full of electrical light " not unlike thofe lights which, in a fummer evening after a hot " day, are feen about the horizon. Even fuch glafs after it has been " removed from the Chain, continues for fome time to afford in- " terrupted flafhes." In that experiment, I only mentioned one

 charge,

charge, whereas there are in reality three, viz. one of the air, and two of the glafs, confequent to that of the air. Let the reader fee the experiment reprefented in Pl. VI. fig. 4. The firft charge is that of the portion of air which ftands between the conductor Y, and the furface near to it of the glafs BCD; the fecond is, that of this fame part of the glafs BCD, to which the immediate action of the electric atmofphere reaches; and the third is that of the glafs in EFG, where it communicates either with me, or any other deferent body. I. The exceffive fire of the conductor flows, in A, into the contiguous air and adheres to it, (I fhall prove in the next chapter that air is capable of taking a charge in the very fame manner as other *infulating* bodies) and drives an equal quantity of fire from the furface of the air which touches the glafs in BCD. II. Therefore, this fire, from the furface of the air, accumulates itfelf on the contiguous outward furface of the glafs BCD; and drives an equal quantity from the inward correfponding furface of the fame: now, it is this fire, which, flowing through the vacuum, emits thofe tranfient flafhes of light we have mentioned. III. The fame fire is determined to run to the inward furface EFG, where it can expel an equal quantity from the outward furface, into the deferent body with which the latter communicates. When I afterwards remove the bottle from the Chain, the flafhes that appear in it are exactly fimilar to thofe that take place in the barometer, and are the confequence of the motion of the fire, which recovers its equilibrium.

CHAP. V.

On the caufes that leffen, or prevent, charges.

268. I Think I have treated pretty largely of the *communication* which muft be between the two oppofed furfaces of an infulating body, and two different fyftems (the one animated, the other indifferent; or both animated, but in a contrary manner *);

* The two fyftems may be both animated, but in that cafe, the charge will only be proportioned to the excefs of the greateft homologous electricity above the other.

in

in order to produce a charge. I likewife think that I have fufficiently demonftrated that the thinnefs of the glafs increafes the intenfity of the charge. I, for inftance, never could fuceeed to introduce any perceivable charge into a Bologna flafk; I mean one of thofe which are of a great thicknefs, efpecially at their bottom, and which break to fhivers by a ftroke from a little piece of a flint dropt into them.

269. But many things ftill remain to be obferved with regard to the *feparation* which muft obtain between the two oppofed furfaces of a plate of glafs, in order to complete its charge. For as this charge cannot be effected but proportionally to the quantity of contrary electricities which accumulate themfelves on the two furfaces of the plate, fo this charge will continually leffen and vanifh in proportion as there will be a communication between thefe contrary electricities; fince, by their thus communicating together, they will mutually deftroy each other, and confequently deftroy the charge of which their *oppofition* conftitutes the effence.

270. Now, the two oppofed furfaces of the infulating body can thus communicate with each other, either through the ambient medium, or through the uncoated edges of this fame body, or even through the fubftance of that body itfelf. Of thefe three kinds of communication, as well as of fuch other circumftances as may obftruct a charge, I propofe now to treat in order, and proceeding by experiments. I fhall afterwards mention the precautions that are to be ufed in order to prevent thefe inconveniences.

271. And firft, with regard to the air, or ambient medium, I rather choofe to appear tedious than forget to mention, that, for want efpecially of a fufficient drynefs in the fame, there are but few days in the year in which thorough charges can be obtained: the fparks are often pretty vivid, and loud enough; but, for all this, the charges do not attain their utmoft intenfity. The experiments muft be made in thofe few days of the year in which a ftrong wind prevails, and the fky is very clear (here, in Turin, fuch dry winds chiefly blow from the Weft, and rumbs adjacent to it): then the fparks of the difcharges will produce effects greatly fuperior, either in melting, or vitrifying, or calcinating bodies, to thofe produced in ordinary weather. A fingle obferation may confirm this: let the

the electroſcope annexed to the Chain is ſeen immediately to attain its greateſt divergency, as ſoon as the inſulating body, which was charging, is removed; and the electroſcope annexed to the Chain is ſeen immediately to diverge to the utmoſt: a moſt certain ſign, this, that ſome moiſture from the air adheres to the uncoated edges of the ſaid body, and was, before the removal of it, as a kind of vehicle, by the means of which the exceſſive fire of the Chain flowed into the outward coating, and thence into the ground. Nor is ſuch moiſture depoſited on the glaſs, only in the ſame quantity as it is ſpread in the atmoſphere, but the electricity draws it, and diſpoſes it in the propereſt manner for it to communicate itſelf through it, as we ſhall ſee when we come to ſpeak of the electrical motions. I have ſaid before how I partly obviate this inconveniency, by placing under the *Franklinian ſquare* ſome hot aſhes; but ſtill I inſiſt, that art never will ſupply, in this caſe, the advantages of a proper ſeaſon.

272. But if moiſture can thus effect a communication between the two coatings of a plate, how can it be therefore, that diſcharges of a pretty great intenſity, are ſometimes obtained in weather imperfectly dry? for it ſometimes happens, that in charging a glaſs, or even an electric battery, that is an aggregation of ſeveral glaſſes, though in cloudy weather, the electroſcope actually reaches its greateſt divergency, and the charge happens to be really complete. I anſwer, that the vapours in the air may ſometimes chance not to form on the margin of the glaſs ſuch a continuous coating, or rather fur, as to enable any conſiderable portion of the electricity to diſſipate itſelf through it; therefore, a ſtrong charge may then be effected, that is, one which is proportioned to the exceſs that has really been accumulated and preſerved. The conſiderable *ſum* of the intervals, however ſmall they may be, between the particles of moiſture, produces in ſuch caſe a ſufficient *ſum* of reſiſtance to ſtop the greateſt part of the electricity which is deſtined, by its accumulation, to form the charge. Thus, I charge a bottle, from the outward coating of which hangs a ſmall chain of ruſty iron; and the diſcharge as well as ſtroke are but weak: the links of the chain, we muſt here obſerve, only touch each other in points, and even in theſe, the contact is rendered imperfect by

an interpofition of the rufty, and confequently *incompletely deferent*, furface: now the confequence of thefe many little refiftances is, that only a fmall quantity of fire is tranfmitted, and the bottle confequently is in the iffue but weakly charged.

273. With regard, befides, to fuch complete charges as are fometimes effected, though the fky may be darkened with clouds, I have obferved that they generally take place during two particular kinds of cloudy weather, viz. in times of temporary tranfient clouds, and ftill more frequently in times of fnowy clouds. When a temporary tranfient cloud paffes and lightens above our heads producing an almoft inceffant noife, and keeping at a little diftance from the ground, in fuch cafe, and before it rains, the electric experiments fucceed pretty well, becaufe thefe clouds being ftrongly electrified, draw to themfelves the other vapours fpread in the air (even in rooms, the air is pretty foon brought to the fame ftate in that refpect, as the outward air) which they incorporate with themfelves, and leave the air that lies near the ground, free from any deferent dampnefs. Alfo, when copious and dry fnow falls, the electricity is extremely vivid. I explained the increafe of the particles, or grains of fnow, by the attraction that takes place between them, when they pafs through clouds unequally electrified. The fine experiment with which Mr. Heberden proves, in the LIXth Vol. of the Philofophical Tranfactions, that of two veffels placed in the fame vertical line, that which is loweft receives a greater quantity of rain than the other, renders it manifeft that rain itfelf increafes in its falling; I fhall introduce in another place fome obfervations concerning rare, and, at the fame time, large drops of rain, which I found to have electrified an umbrella of tin, infulated. Now, fince we know that kind of force which produces the mutual attraction and agglomeration both of the particles of hail and rain, why fhould not fnow likewife draw the vapours diffufed in the lower part of the atmofphere? a drynefs even greater than in the former cafe, muft in this cafe obtain, fince fnow can reproduce no new moifture in the air.

274. I have hitherto mentioned only that moifture in the air, which fpreads itfelf on the uncoated margin of the infulated body, and produces the abovementioned decreafe of the electricity in the

Chain

Chain at that inftant, when the infulating body, deftined to be charged, is applied to it; but the electricity in the Chain can befides receive a diminution from any dampnefs that may take place in the bodies that are made ufe of to infulate it, fuch as filk ftrings, pieces of glafs, or oiled wood, and efpecially in the ambient air: fo that the electrofcope may then, even though the infulating body be not annexed to it, rife but little, and the intenfity of the other figns be proportionally diminifhed.

275. If the dampnefs were copioufly diffufed in the air, the figns might even fail intirely. Attempting once to make experiments in a place in the country which was furrounded by growing plants, I found I could not fucceed to excite any ftrong electricity; which I attributed to the copious tranfpiration of vegetables. The fame will happen in all fmall confined rooms, where many perfons are prefent. If fire be lighted, it will drive out the damp air, and draw in new and dry air, by which the electricity may be kept up with fome degree of vigour. It is only to the copious dampnefs he muft have produced, that I impute that Dr. Prieftley, after filling a glafs bell with air that had paffed through his lungs, and immerfing in it a charged bottle, faw the fame difcharged in a lefs interval than two feconds (Hiftory of Electricity, p. 599.) The moift vapour, of which the air in the glafs bell was replete, became a conductor continued enough to enable the charge to pafs from the hook, to the coating of the bottle; though, at the fame time, the intervals, however fmall, between the particles of the deferent vapour rendered the paffage of the fire flow enough to prevent a fpark. When I want to fupprefs a charge for which I have no farther occafion, I lay the bottle on an old table of walnut-wood, fo that the hook touches the table, and it difcharges itfelf there filently, though fpeedily, and without giving me any farther trouble; the remnant of deferent particles in this table is fufficient to diffipate the charge, but their feparation, as well as nature, occafions fuch difcharge to be only filently effected.

276. Dr. Prieftly has filled the fame glafs bell with air proceeding from a fire of wood; and a bottle immerfed in it has been in a like manner difcharged: nor has the fame gentleman ever been able to charge the faid bottle when keeping it immerfed in fuch

air : but the air being changed, the bottle kept its charge if already charged, or received one, if it had none. (Ibid. p. 600.) Dr. Prieſtley has found afterwards that coals of wood, and pit-coal, were conductors of electricity. He calls them, excellent ones; to me they have appeared inferior to metals, becauſe though they may conduct the ſpark to the ſame diſtance as they, and leave no greater remnant of charge; yet, they do not conduct the ſpark ſo united and compact; the lightning of it is not ſo vivid, nor the crack ſo loud, as when it is conducted by metal.

277. Whatever may be the real conſequences to be drawn from Dr. Prieſtley's experiments, I think, I perceive from them, a new kind of analogy between the common and the electric fire, as well as diſcover a probable explanation of a very much known phenomenon, concerning the former; which is the gradual extinction of a flame in air impregnated with vapour from the lungs, or from burning coals, or in any other mephitic air. Since my object in this book is to promote the knowledge of Nature, it will not be amiſs, I think, to expoſe here how I explain the gradual extinction of a flame in air that grows continually more and more dilated. I conſider ſuch an extinction of the flame as a real diſſipation of it :—in order to enable the flame to live, it is neceſſary that a certain portion of it be continually repercuſſed, and kept cloſe to its *pabulum*, in order that it may looſen, and continually put in motion, new particles of it, and diſcloſe the fire impriſoned in them, in order to ſupply that which continually diſſipates. Now, air is the medium that repercuſſes and confines ſuch portion of fire : in proportion as air grows dilated, the fire is the more copiouſly diſſipated; only a leſs portion of new one is then diſcloſed; the flame thus gradually leſſens, and at laſt fails, after draining the cavity of the candle around the wick. Now ſince air, impregnated with vapour, either from the lungs, or from burning coals, is able to conduct the electric fire, why ſhould it not be able likewiſe to conduct common fire? this circumſtance, if it were well aſcertained, would give a new degree of extent to the already known analogy between thoſe bodies which are *deferent* of both the ſaid elements; though we muſt obſerve, they perhaps are but one and the ſame element, which is only differently combined.

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Farther, ſince a flame dies away in rarefied air, in conſequence of its diſſipating too much in it, why ſhould it not likewiſe die in air filled with vapour from the lungs, ſince through them it can as well diſſipate, as through a vacuum? But of this we ſhall ſay more hereafter.

278. Now it is neceſſary that I ſhould proceed to conſider the different kinds of communication which (beſides that from moiſt air, or any deferent medium) may be afforded by the ſurface of the inſulating bodies on their naked margins: I think that they may be reduced to four principal ones, viz. to a too great narrowneſs of theſe margins; to a want of cleanneſs; to a defect in the ſhape and clipping of the coatings; and to the bad condition of the deferent body, which may prevent it from inſulating exactly.

279. With regard to the three firſt caſes, we ſhall ſay little. 1. For the charging of the largeſt batteries, a margin of three inches on each ſide of the inſulating body is ſufficient; this being a diſtance which no charge, ever ſo ſtrong, has yet being known to be able to go through. 2. But ſuch margins muſt moreover be exactly inſulating; that is to ſay, clean. Whatever particles the electric fire may meet with, which it can drive into its own way, it will actually drive, and will make them, as it were, ſo many points of ſupport, by the means of which it will leap, and ſucceſſively reach, to a great diſtance: this I have ſhewn before in my Book of *Terres Atmoſ. Elec.* and will more amply ſhew hereafter, in the chapter on the Electric Sparks: I therefore again inſiſt, that the naked margins muſt be kept exactly clean. 3. In order that the aforeſaid diſtance of three inches may ſuffice, care muſt be had that the angles of the coatings be not ſharp, elſe they will throw out particles of fire. See, for inſtance, what takes place in the coating of one of thoſe plates, *f*, *g*, *h*, *i*, which I call *Franklinian ſquares*, when charged in the dark: in proportion as the charge advances, bruſhes of light ſpring to a great diſtance from each of the angles *f*, *g*, *h*, *i*, and fringes of light line the ſides of the ſaid coatings: now, all this quantity of fire is ſo much that is taken from the charge. This inconveniency we may indeed remedy by rounding the angles of the coatings; but the beſt remedy of all is to apply

apply around them a coat of fealing wax; thus the diffipation of the electric fire through the fharp fides, and much more through the angles, of the coatings, will be in a great meafure prevented, as we fhall fee hereafter, when we fhall treat of the electric *brufh* and *ftar*: whatever plate I have fitted thus, has been able to receive a ftronger charge than others of a fame fize. The abovementioned brufhes and fprings are by no means a portion of fire ufelefs to the charge: in fact, long after they begin to appear, the charge ftill continues to increafe, and no doubt but it would increafe ftill farther, and at leaft be formed much more fpeedily, if fuch diffipation of the fire were prevented beforehand. The care of fmothering, or fuppreffing, fuch brufhes of light, muft alfo be extended to the whole body of the Chain, of the rod, and any other body, by cutting or filing off all prominent parts on them. Any moveable and deferent particle on either of the coatings, would alfo greatly leffen the charge: an old experiment of mine may here ferve as an example: I had placed a ftripe of tin-foil clofe to the coating of a plate; and when the fpark rufhed to the *conducting* bow, it carried the ftripe along with it, and diffipating it in fmoke, imprinted fome of its particles on the bow.

280. But the principal caufe of the diffipation of the electric fire, is the fourth we have mentioned; viz. the bad condition of the infulating bodies: which, in fome, is perpetual; in others, tranfitory. It is perpetual in fome glaffes in confequence of the particular ftuff of which they are made; and tranfitory in other glaffes, which, though their fubftances may be excellent, yet do not infulate, either becaufe they are new, or, though they may be old, have been lately heated fomewhat intenfely.

281. Dr. Prieftley (p. 616) coated a bottle of blue glafs, near its bottom, to the height of an inch and a quarter; the bottle was fix inches and an half high, and two inches and an half wide; and, as the charge was advancing, the fparks began to fly from fuch naked places of the glafs as were near the coating: then they fucceffively flew from higher points under the fhape of brufhes that became gradually larger; and laftly, they ran from the infide of the bottle along the outfide of it; and thus the difcharge was ef-

effected. The bottle was afterwards alfo difcharged, though the coating was reduced to half an inch: and even another fimilar bottle filled with filings of iron, was difcharged without any other coating than a little iron chain which faftened it to the table, near its bottom.

282. Dr. Prieftly obferves, that the firft of thefe bottles which was broke only eight months afterwards, retained ftill the fame property: I fhall obferve here, that the progreffive motion of the fparks on the uncoated furface of bottles like thofe mentioned here, till a difcharge is effected, only differs in point of quantity and fwiftnefs, from the ordinary phenomenon obferved in the charging of bottles, the furface of which are not, in all their parts, equally deferent. When, for inftance, the bottle A B is charging at the Chain, (Pl. II. fig. 5) little fparks foon begin to iffue from fuch parts of the glafs as ftand near the coating; which fparks fucceffively grow larger, and fparkle from the diftance of half an inch, an inch, an inch and an half, or two inches, and even fometimes more; but for all that, a difcharge does not follow, becaufe the glafs, if it be good, well polifhed, and dry, is not able to tranfmit the fpark to any greater diftance. To render the experiment ftill more confpicuous, I ufe bottles with a long neck, and I coat them with a ftratum of cement, which is a fubftance ftill lefs deferent than glafs; to which add, that fuch an addition to the former thicknefs of the bottle, will leffen much the inward force which drives the fparks from the outfide of it: now, it is this very force which is the caufe of that phœnomenon which Dr. Prieftley calls a very beautiful one. The exceffive fire, in proportion as it accumulates itfelf on the inward furface of the bottle, correfpondently to the outward coating of it, fucceffively exuberates, both in the filings, and in the iron wire ufed by Dr. Prieftley, where they refpectively correfpond to the outward uncoated furface: confequently, fuch fire in proportion as its denfity increafes, drives portions of fire from fucceffively remoter places of the uncoated outward furface, into the coating of the bottom of the bottle. And as to the bottles ufed by Dr. Prieftley, made of a glafs the deferency of which is of a particular nature, the fame exceffive fire, when it has attained its greateft denfity, becomes able to rufh, one part of it, into

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the outward coated ſurface, which is deprived of the greateſt part of its fire; and, the other part, into the uncoated ſurface, into which a deficiency has been raiſed, proportioned to the ſparks that have alſo been at times driven from it.

283. The ſame explanation may ſerve likewiſe to illuſtrate, if not the cauſe, at leaſt the manner after which thoſe phenomena are effected, that are obſerved to take place in thoſe glaſſes, which, on account of their being new, cannot, for a while, inſulate but in a very inconſiderable degree. When I examined at firſt the experiment of the abbé Nollet, deſcribed in fig. 9. pl. III. I inſerted an old bottle B, in the neck S of an old glaſs bell C E D, and I remained not a little ſurpriſed that I could not perceive within this bell (while indeed all the reſt of the experiment ſucceeded extremely well) that great quantity of fire which the abbé ſaid filled the recipient, during the time that the bottle was electrified, and moved in it with a ſtupendous rapidity: in a word, I was unable to diſcover any thing like the beautiful, and, indeed, true delineation that he gives of ſuch phenomenon, in the firſt plate of the firſt volume of his Letters. I procured afterwards another glaſs bell from the furnace; the height of it was a full foot, and it was conſequently more nearly equal to that uſed by the abbé; and when I repeated the experiment, I ſaw flaſhes of light like thoſe obſerved before by him, winding about in a moſt extraordinary manner: but the very ſame evening, having applied my hand to the glaſs bell, while with the other I tried to effect a diſcharge, I was vehemently ſtruck, and the bell broke, it being made of a thin glaſs. I procured another of the ſame ſize, which exhibited to me the ſame ſpectacle; and I laid it in a place where, upon occaſion, I could find it again.

284. But I muſt confeſs I was amazed, when about a year after, intending to repeat the ſame experiment, I found myſelf unable to raiſe thoſe ſtrong and winding flaſhes that before filled the glaſs bell; and I could not ſee any thing more than an almoſt ſingle ray, which, from the outward bottom of the bottle, flowed to the knob on the plate of metal on which the glaſs bell lay. Then, for the firſt time, the thought occurred to me that white glaſſes, when recent, are in ſome degree conductors of the electric

 fire;

fire; the reaſon I had for thinking ſo, was, that the flaſhes, which moved in a vermicular line, did not poſſibly flow through a vacuum, ſince, as I had before obſerved in numberleſs experiments, they always proceed in it, in a right line, or evenly incurvated line: the vermicular motion of the flaſhes, juſt mentioned, muſt therefore proceed from their running in a contact with the interior ſurface of the glaſs, where they follow ſuch crooked path as is afforded them, by ſuch parts of the glaſs as actually are, or ſucceſſively become, deferent, in conſequence of the electric fire affixed to the parts next to them. I imagined beſides, that this defect of new glaſſes becomes, in a courſe of time, gradually leſs; and that this was the reaſon why the flaſhes failed within an old glaſs bell, and afterwards in the new one, when I had left it grow old. I thought that this defect was confined to white glaſſes; becauſe cylinders, or globes, of greeniſh glaſs, the ſame as that of which bottles are made, though they were newly made, had ſerved me very well at all times to excite electricity. With reſpect to this my opinion, I think, I pretty well agree with Dr. Prieſtley, who has afterwards made many experiments relative to the *deferent* quality of recent glaſſes. He has eſpecially made his experiments with tubes of flint-glaſs, and has found them, in ſome degree, deferent, when they were new. I. Whether they were thin, or thick: II. whether they were poliſhed, or rough: III. whether they were hollow, or ſolid. IV. The ſame gentleman has found, moreover, that glaſs, however recent, becomes continually more and more deferent, by being ſeveral times charged and diſcharged; but that, after a few hours time, it returns to its former ſtate: V. that a ſpace of a few months, after the glaſs has been taken from the furnace, and uſing it, are neceſſary to render it inſulating: VI. that, when they are once become inſulating, they may again be rendered deferent, by being expoſed to the action of a fire ſufficient to render them ſoft and pliable. (P. 621.)

285. I generally uſed to beſpeak tubes of glaſs, two or three yards long; I took them hot from the furnace, inſulated them, and found that at the ſame time that I applied a charged bottle to the end of one of them, the electroſcope, annexed to the other end, diverged, and a pretty ſtrong charge was communicated to another bottle, annexed to the ſame end: but, a few months after, I found

that the fire did not diffufe itfelf through the fame tube, to a greater diftance than half a yard.

286. I applied a coating, three inches broad, to the middle of a tube, open at both its ends, and a yard long; I charged the coated part of it by the means of an iron wire, when it feemed as if a flame fprung out from both fides of the tube; and fparks ran to my fingers, as I moved them along it, from all the fucceffive parts of its furface.

287. I took another tube, an inch in diameter, three feet and a half long, and extremely thin, and applied to it a coating three inches broad, at the diftance of nine inches from the one end. When the coated part was charged, the whole furface of the glafs threw fparks to the diftance of an inch. In moving my hand along its outward furface, I deftroyed its vifible electricity. By prefenting my finger to one of the two openings, I diminifhed the inward electricity, and again excited the outward one; and, by proceeding thus at feveral times, I effected a difcharge by *alternation*.

288. The fineft experiment of Dr. Prieftley has been that, in which he applied a coating, three inches broad, to the tube A B, in the fame manner as is expreffed in the Pl. VI. fig. 3; then inferted an iron wire E, in contact with the interior coating, and charged it by joining the wire to the Chain. This done, he took the coating C D, and moved his other hand along the tube from B to D, as if he had intended to rub it. When his hand came to be diftant from the coating only two inches and an half, a fpark flew from the latter into his hand, which produced a fhock: fometimes Dr. Prieftley was obliged to move his hand, even three times, from B towards D, before he could obtain a ftroke; at other times he was ftruck at the firft motion; and fometimes he even received a farther ftroke at his third motion.

289. In order to explain this kind of ftrokes, Dr. Prieftley made ufe of a fmaller tube, within which he had made a perfect vacuum (this experiment is the fame as that which I have explained in No. 486, 518, of *Artificial Electricity*), and pretended that the charge and difcharge of that tube being explained, would throw a light on the new method of obtaining a fhock from tubes in fome degree deferent, within which no vacuum had been effected. The

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truth is, that ſuch a diſcharge is only a diſcharge by *diſtribution.* I take a tube A B, which is exactly inſulating (Pl. VI. fig. 3.); I charge it in C D, after having coated it in the ſame manner as Dr. Prieſtley did his own: then I apply my hand in F, and another perſon puts into the opening B of the tube, a rolled piece of tin-foil, like that uſed for looking-glaſſes; this piece of tin-foil is made to move forward and backward, within the capacity of the tube, by the means of a ſtick of glaſs to which it is faſtened. This done, two ſparks are thrown, the one inwardly, from the inward coating C D, toward the piece of tin-foil (which, as I ſuppoſe, is made to accompany the motion of the hand on the outſide); the other, from the hand to the outward coating. By the former ſpark, the exceſs becomes divided, that had been at firſt introduced into the inward coated part; and by the latter the deficiency of the charge raiſed on the outſide of the ſame coated part, is leſſened. In fact, if I take my hand from F, I find the tube (the exceſs introduced into its inſide being a little diminiſhed) electric in that place by deficiency.

290. I think, the ſtroke produced by Dr. Prieſtley was of the ſame kind. I. He introduces, into a recent tube in C D, outwardly an *exceſs*, inwardly a *deficiency*. The hand applied to F, enables a portion of natural fire to run to D C, and thus to leſſen the deficiency in it; becauſe the exceſs, which is raiſed on the inward ſurface in D C, then becomes able to diffuſe itſelf into that part of the ſame inward ſurface which lies next to it and correſponds to the hand outwardly applied in F. But we muſt obſerve, that ſuch diſtribution of the inward exceſs is facilitated both by the nature of the tube, which is in ſome degree deferent, and by the motion of the hand along its outſide, from B to F; the effect of which motion is, that the exceſs in B can run to the hand, while the latter ſtands near; and the exceſs that remains in thoſe parts which are neareſt to F, can inwardly flow towards B, as the hand moves towards F.

291. We may, however, conclude this from the above experiments, viz. that recent glaſſes are able to retain a certain degree of charge, and that they conſequently do oppoſe ſome reſiſtence to the fire that endeavours to flow along their ſurface.

That ſuch reſiſtance becomes ſucceſſively greater, in proportion as thoſe glaſſes grow older, and are more uſed; ſince a greater force and denſity of the electric fire is then required to ſurmount it. The conſequence to be drawn from all this is, therefore, that glaſſes then loſe leſs of their fire; loſe it only at a ſmaller diſtance, and can retain ſtronger charges.

292. Dr. Prieſtley concludes his ingenious ſection with the following experiment. He put a ſlip of gold or braſs leaf between two tubes, the one old, the other new, and ſtrongly faſtened to each other; and obſerved that, when he had happened to draw very ſtrong ſparks, the metallic particles were more indelibly impreſſed on the new tube than on the old one, and that ſuch impreſſion was manifeſted by a more beautiful colour: which difference, he conjectured, was owing to a difference in the pores, which were more open in the new glaſs; and as the ſame grows old, become ſucceſſively contracted.

293. But to ſpeak of what I have myſelf obſerved, I ingenuouſly confeſs, that the fineſt experiment I have been able to ſee, with regard to the deference of recent glaſſes, is that which I have deſcribed in numb. 285, that of the vermicular flaſhes which filled the bell made of recent glaſs, which was extremely brittle, even beyond what might be expected from its thinneſs. In other white, or greeniſh glaſſes, I could not perceive ſuch a great deference as Dr. Prieſtley has obſerved in his cryſtals.

294. I beſpoke ſeveral tubes of white glaſs, and ſome of them were ſcarcely made when I expoſed them, ſtill hot, to the open air, which was cold to the degree of congelation; and a few others of the ſame ſize I got to be heated again in the furnace. I tried them all the ſame day, and found that they diffuſed away, in ſome degree, the electricity of the Chain; but this they did not, either with ſo long flaſhes, or ſo plentifully as the tubes of Dr. Prieſtley did: though I obſerved that ſuch tubes, as had been heated twice, loſt their electricity in a leſs degree than thoſe which had not been. I have repeated the ſame experiment with tubes of greeniſh glaſs, the one heated a ſecond time, the other not, and the effect has conſtantly appeared to me to be the ſame.

295. Dr. Prieſtley ſays, at p. 622, that ſome tubes, that had been made the ſame day could not be electrified, though rubbed ever ſo much

much with the amalgamated cloth. With me, quite recent tubes have become a little electrified, when rubbed; and if a little heated, have manifested a pretty considerable degree of electricity; though they only retained it for a very short time.

296. I say, a little heated, because a great degree of heat is itself the last of those transitory causes mentioned above, that render glasses *deferent* on their surface; and is almost the only one that makes them pervious to the electric fire, in their inward substance. Before I examine the effect produced by this last cause, viz. heat, I will explain why I say, it is *almost* the only one that renders the substance of glass pervious to the electric fire. Ever since the experiment of Leyden has been known, attempts to charge thin Florence flasks, or large matrasses, have always proved unsuccessful: I attributed it at first to the number of bubbles remarkable in that kind of glass, which, by dividing the thin substance of it, left some continuous openings in its inside, that gave an access to the fire. But the opinion of Mr. John Canton, on that subject, is no less probable: he thinks that those little spots on the glass are formed by metallic particles, which, not being sufficiently melted, afford a passage to the subtle elements. Indeed, it does not appear that thinness may of itself be the cause of the penetrability of glass: I have blown with a lamp small bottles of glass, and made them as thin as they possibly could be, and still they were able to receive most intense charges.

297. Let us now examine the effects produced by an actual degree of heat in the glass: in the Letter I wrote to Dr. Franklin, the 20th of February, 1767, I expressed myself in the following terms, in numb. 33. *When I try to charge a glass very intensely heated, sparks are continually thrown from all parts of it: we must therefore conclude, that the fire either circulates around such glass, or passes through it: I have an experiment ready to ascertain this.* Here is then the experiment, as I made it. I procured a bottle B C D E (Pl. VI. fig. 5.), the neck of which was crooked, and eight feet long; I put water into it, so as to fill only a sixth part of its bottom, and made an iron wire dip into it. I insulated the same by two strings of silk, fastened to it in C and D; I shook the bottle, that the water in it might wet all the inside of it, and

made

made the hook communicate with the Chain. When I grafped the bottom of the bottle, the electrofcope rofe to a great divergency; and when I afterwards touched the Chain with my other hand, I received a ftrong ftroke. I then put a lamp under the bottom of the bottle, which foon made the water in it boil, and, in fuch ftate of things, the electrofcope diverged no more. I infulated the lamp F, when the electrofcope immediately diverged; and at the fame time, I could continually draw fparks from the metallic prop of the lamp; but when I intirely touched it, the electrofcope immediately loft its divergence. This is to be concluded therefore, viz. that the electric fire paffed through the bottom of the bottle, when heated to the degree of boiling water. It was only after making this experiment, that the fimilar one, of which Mr. Kinnerfley wrote a defcription to Dr. Franklin from Philadelphia, came to my knowledge. "I put," fays he, "boiling "water into the coated Florence flafk, and found that the heat "fo enlarged the pores of the glafs, that it could not be charged. "The electricity paffed through as readily, to all appearance, "as through metal (*a*): the charge of a three pint bottle went "freely through, without injuring the glafs in the leaft. Would "not this experiment convince the abbé Nollet of his egregious "miftake? For when the electricity went fairly through the glafs, "as he contends it always does, the glafs could not be charged "at all."

298. Even before I had received the complete edition of the Works of Doctor Franklin, I had heard of the experiment made by lord Cavendifh, related in page 403 of them. I do not think, I have perfectly well underftood the particulars of that experiment: however, I fhall relate in what manner I have tried to imitate the effential part of it, and, if I do not miftake, carried its confequences even farther. I. I took a fmall tube of glafs C D (Pl. VI. fig. 6.) of white glafs, three lines in diameter, the fifth part of a line thick, and three feet long, through which was paffed an iron wire G H. I then made the tube go through the two pieces of pafte-board that ferved to clofe on each fide the fmall veffel of brafs A B,

(*a*) We fhall fee, however, that though the degree of heat be higher than that of boiling water, glafs never is fo deferent as metal is.

which was two inches broad on every ſide. II. That is to ſay, I placed the two pieces of paſte-board (they are not expreſſed in the figure, to prevent a confuſion) in the vaſe A B, in ſuch a manner, that the tube paſſed through them, and alſo through the two openings, made, facing each other, in both ſides of the veſſel; and then I moved the two ſquare pieces of paſte-board from each other, ſo as to make them cover pretty exactly the two openings. III. This done, I introduced into the veſſel the ball of a thermometer emptied of air, which could riſe to number 255 above o (*a*); and, keeping the ſame ſuſpended, ſo as to have its ball on a level with the loweſt ſide of the tube, I poured into the veſſel as much iron filings as were neceſſary to cover the ſaid ball. IV. I made the iron wire G H, that ſerved as an interior coating to the tube, communicate with the Chain, and annexed an electroſcope to it in H, which I call the electroſcope of the interior coating. Laſtly, I faſtened to one ſide of the veſſel the iron rod I K L, ſuſpended by a ſilk thread, to which I annexed another electroſcope, which I call the electroſcope of the exterior coating: becauſe the rod I K L, the veſſel A B with the iron filings, and the table on which the whole lies, which is inſulated, all belong to that coating which covers the outſide of the tube.

299. Things being thus diſpoſed, I. I excited the electricity of the Chain, and in the meanwhile I now and then touched the veſſel, but after four or five ſeconds, could no longer draw ſparks from it; and the electroſcope L, of the outward coating, which at firſt was made to diverge by the natural fire that was driven from the outward ſurface of the tube, abſolutely ceaſed to do ſo, though I continued to touch the vaſe; that is to ſay, in four or five ſeconds the charge was completed.

300. II. I then put under the vaſe a little lamp with ſpirit of wine, when I was not a little ſurpriſed to ſee that, after a few ſeconds, when the thermometer from 8 degrees (Farenheit's grad. 25 deg.), where it ſtood at firſt (the experiment was made the 17th of November, though a moſt excellent weather), had ſcarcely

(*a*) The author ſpeaks here of a thermometer graduated after the method of Monſ. de Reaumur; the number 255 of which amounts to number   of Farenheit's ſcale.

got

got to nine, that the electroſcope L began again to diverge a little, while the electroſcope K of the inward coating manifeſted a tendency to fall. I ſeveral times tried to aſcertain this circumſtance, by repeating the experiment; and at every time I found that the addition of a ſingle degree of heat is ſufficient to make a charge already completed, riſe ſomewhat higher: which may be owing to two different cauſes; either ſuch a ſmall increaſe of heat enables new and remoter parts of the ſurface of the tube to give ſome of their electric fire; or, this ſame ſmall degree of heat enables that very part of the glaſs, which was at firſt as much deprived of fire as it then could be, to yield ſome additional one.

301. That the charge ſhould increaſe after the manner firſt mentioned, is intirely conformable to experience: yet it appears to me no leſs probable that it increaſes alſo after the ſecond manner, to wit, that ſucceſſive additional degrees of heat enable continual new electric fire to leap from other more and more internal pores of the ſame part of the glaſs. Such hypotheſis ſeems to be conformable to the univerſal law of *ſimilar actions*, which never proceed but gradually and evenly through the ſubſtance of bodies; and it moreover particularly agrees with the nature of glaſs, which oppoſes the diffuſion of the common fire in the ſame manner as we ſee it does that of the electric fire, and is not penetrated by it, but gradually and ſlowly: ſo that the dilatation of thoſe ſtrata which the fire has already penetrated, cauſes thoſe which are ſtill unpenetrated and contracted, to break. Such hypotheſis would alſo ſatisfactorily explain the effect of the method uſed by ſome perſons who, in order to increaſe the charge of certain bottles, fill them with water a little warm: it might in ſuch a caſe be ſaid, that ſuch ſmall degree of heat cauſes the electric fire to be more copiouſly and deeply condenſated than it was before, within the pores of the one ſurface, and alſo to be driven more abundantly, and from deeper parts, from the other ſurface; without, for all this, paſſing through the ſubſtance of the glaſs, or at leaſt without paſſing in ſuch quantity as to compenſate on the one ſurface, that greater quantity which is accumulated on the other. But that which particularly confirmed me in this hypotheſis, was the farther progreſs of the experiment; becauſe, III. it always happened

pened that, as the heat increafed, the divergence of the electrofcope L of the exterior coating increafed, whenever it remained untouched; and whenever the fame was touched, the divergence of the electrofcope H diminifhed.

302. IV. When the mercury was not above the 20th degree, even then, if the vafe was frequently touched, very fenfible fparks were thrown out, and in thofe inftants the electrofcope H was falling down. To explain thofe circumftances fhall we fay, that they were not owing to the charge which did only reach fucceffively deeper, but to the fire that fairly paffed through the fubftance of the glafs itfelf? Yet, charges often fucceed, and are exactly completed (that is, fparks from the outward coating are often feen intirely to ceafe) in fummer days, though the thermometer rifes to 22 degrees, or more; that is, to a degree of heat fuperior to that which obtained in the above experiment.

303. V. From this experiment I could conclude no fure method of determining nearly enough, at what degree of heat the electric fire really traverfed the fubftance of the glafs. At 25 degrees, when I conftantly touched the outward coating, the electrofcope H of the interior coating was already confiderably lowered, and only rofe again to its greateft divergence, when I left touching; and then, in four or five feconds, the electrofcope L acquired a divergency equal to the greateft of the electrofcope H.

304. VI. This at leaft I have concluded, viz. that a degree of heat, much beyond that of boiling water, is requifite to render a glafs as pervious to electric fire as metal is. When the action of the flame had lafted twelve minutes, the mercury rofe to 140 degrees; yet, when I then touched the exterior armature, two feconds were neceffary for the electrofcope H to lofe its divergence; nor did it even completely lofe it, though I conftantly touched the faid coating, but always was fomewhat attracted when I prefented my finger to it: whence we muft conclude, that even at fuch degree of heat the electric fire did not freely go through the glafs.

305. Every part of the apparatus being grown cool, I fubftituted another tube made of the fame kind of glafs, but of a thicknefs double to that of the former; and I then found, I. That the charge, when every thing was in its natural ftate of coldnefs, was foon

completed; that is to ſay, in two or three ſeconds, conformably to what has been obſerved before, that thick glaſſes can only receive a leſs charge. II. The ſparks from the exterior coating have taken more time to appear with a certain degree of vividity. III. And laſtly, it only was after twenty-four minutes of a continual heating, when the mercury had riſen to 190 degrees, that it appeared to me that the fire paſſed freely enough through the ſubſtance of the glaſs: ſince that time, whenever I continued to touch the outward coating, the electroſcope H, of the inward one, loſt nearly its whole divergence.

306. Laſtly, I tried the ſame day a third tube, a twelfth part of an inch thick, and of a greeniſh kind of glaſs; and I became ſtill more confirmed in the opinion, that glaſſes of different degrees of thickneſs never are traverſed by an equal quantity of electric fire, but when thoſe, which are thickeſt, have contracted a greater degree of heat than the others: in fact, the mercury had already riſen to 200 degrees, when, though I conſtantly touched the electroſcope L, the electroſcope H ſtill retained a pretty conſiderable divergence.

307. When I attempted at firſt this kind of experiment, I uſed ſhort tubes, and a rod K L, that likewiſe was only ſome few inches long: I likewiſe made uſe of a lamp with oil of olives, and made the experiment in a cloſe room: each of which circumſtances obſtructed the ſucceſs of the experiment; for, even though I continued touching the outward coating, the electroſcope L, annexed to it, continued to diverge. I obſerved that, when I began to touch the coating, the electroſcope firſt fell down, and then began to diverge again, though I ſtill continued to touch; whence I perceived that it did not diverge in conſequence of its own electricity, but becauſe it was immerſed in vaporous electrified air. I concluded, therefore, that a flame from ſpirit of wine, by yielding vapours leſs electrifiable, would be fitter for my purpoſe: that, by uſing longer tubes, and a longer rod I K L, the electroſcope L would be more diſtant from thoſe bodies that electrified the vapours in the room; and laſtly, that, by keeping my inſtruments in the open air, all ſuch vapours and electricity would be prevent-

ed.

ed. And it has only been, after all these precautions, that I could bring the experiment to its neceſſary ſimplicity.

308. But let us now draw ſome uſeful rules from all theſe obſervations and experiments. Certainly, if to the above mentioned effects of heat on glaſs, we join what has been explained and diſcuſſed in this chapter and elſewhere, I am greatly miſtaken, if it does not give much aſſiſtance for the ſolution of the important problem of producing the greateſt poſſible charges. I. Let an apparatus be procured of a Chain, Machine, and glaſs, that be moſt active, that is, that can procure the greateſt poſſible quantity of fire in a given time. To that end, an apparatus of glaſs plates will be very uſeful, if it can be procured ſo as that each plate performs its own function with a ſufficient degree of exactneſs, according to what has been ſaid in number 52. II. Let glaſſes, or cryſtals, be choſen that be old enough, in which there be the greateſt poſſible capacity and thinneſs, taken together; and in which a ſufficient amplitude be combined with the leaſt *perimeter* poſſible of uncoated margins: the reaſon of the latter caution is, becauſe glaſſes, like air, ſeldom inſulate thoroughly; therefore, the leſs the uncoated margins are, the leſs the number will be of points through which the two contrary electricities may communicate, and thus deſtroy each other. This is one of the reaſons why in electric batteries the ſtrength of the charges does not increaſe proportionally to the number of the glaſſes that are uſed, and why a veſſel with a long narrow neck receives more vehement charges than a plate, even of a greater capacity: to this reaſon I have imputed thoſe moſt vehement ſparks, which I could draw from the ſtratum of cement with which I had coated the globe of braſs (Pl. IV. fig. 5.). III. Theſe margins muſt, beſides, be kept extremely well poliſhed; and when either they, or the other parts of the glaſs are to be warmed, care muſt be taken not to render them ſenſibly pervious to the electrical fire. IV Armatures or coatings muſt be uſed, that are in the higheſt degree deferent, conſequently made of metal; they muſt beſides, (in thoſe places particularly whence the ſpark is to be drawn), be of a ſufficient, ſolidity, ſo that the heat of the metal, which would otherwiſe be melted, do not injure the glaſs: the angles

muſt, moreover, be clipt, or, which is better, the perimeter muſt be lined with an edge of ſealing wax, where it lies expoſed to the open air (we ſhall ſee, in its proper place, what is to be underſtood by the words *open air)*; and care muſt be had, that the ſyſtems, with which the coatings communicate, and which are therefore as it were a prolongation of the latter, be freed from any ſharpneſs or aſperity on their ſurfaces. V. The two oppoſed coatings muſt communicate with the two contrary ſyſtems, the Machine, and the Chain, that the two contrary electricities may concur in increaſing each other.

309. It is true, Dr. Prieſtley, in page 584 of his Hiſtory, indicates a particular method of increaſing the charges. "I charged (ſays he) a tube about three quarters of an inch in diameter, and coated about eight inches, in the glaſs veſſel, containing about two atmoſpheres,; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flaſh, twice as great. At laſt the tube burſt by a ſpontaneous diſcharge, after being charged and diſcharged three or four times, in the condenſed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air." But with regard to me, whatever diligence I have uſed, I could not ſucceed in the experiment above. Let the apparatus be the ſame as is expreſſed in the Pl. VI. fig. 7: it conſiſts of an ample cylinder of glaſs, open at both its ends; it is placed ſtanding, and a bottom of braſs is faſtened to it, with a ſolid ring of metal, and ſome very tenacious cement: to the ſuperior opening let another very maſſive ring of braſs be likewiſe faſtened with cement; this ring muſt be ſo made, as to be opened and contracted at will, with a ſcrew, and it will ſerve to faſten on the ſame opening a piece of leather, with a lid of braſs: from the middle of this lid riſes a ſtrong tube of braſs, ſupplied with a little key, to which is adapted the *compreſſing ſyringe*. Before I place the ſaid lid, I put a jar of cryſtal, coated on its outſide and inſide, within the cylinder, as may be ſeen in the figure: this jar is placed on a ſheet of lead, which makes it communicate with the bottom of the tube, and, at the ſame time, prevents its receiving any injury from a ſhock, or

other accident: the jar is beſides ſecured, laterally, by ſhavings of the ſame metal. When the lid has been placed on the cylinder, I open the top of the tube of braſs which riſes from it, and let down through it a thin iron wire, which is of a ſufficient length to touch, with one of its ends, the bottom of the jar, and, with the other, lean againſt the inſide of the opening of the tube. I have a number of times charged and diſcharged this veſſel, thus incloſed, ſometimes leaving the air around it in the natural ſtate, ſometimes doubling, or even increaſing ſtill more its denſity; and in all theſe caſes the charges and diſcharges have appeared to me equally ſtrong, and the light from them to exhibit exactly the ſame appearances, either with regard to its ſparkling, or other motions. I have charged, at the ſame time, both the jar incloſed as above, and another glaſs of the ſame ſize and form, placed in the open air; and I have found that their two charges exactly balanced each other, which muſt neceſſarily be the caſe: whence we muſt conclude, that the electric fire cannot, though incloſed in condenſated air, acquire a denſity ſuperior to that of the fire, in a veſſel which ſtands in the open air. It is very probable, that Dr. Prieſtley has made his experiment in extremely favourable weather, and the intenſity of the charges which he obtained, have induced him to think that the charge of that glaſs, which ſtood in the condenſated air, was doubled; and his having omitted to make the compariſon, like that juſt mentioned, between the two charged jars, gives a new degree of probability to this conjecture.

CHAP. VI.

On the preſervation of charges, and increaſing of diſcharges.

310. WHEN, in the year 1753, I wrote to the abbé Nollet, concerning the imperviouſneſs of glaſs to the electric fire, I ventured to tell Count Carburi, that I hoped I ſhould once be able to ſend to Paris a bottle, hermetically ſealed, and charged: but I happened, at that time, to operate in a very hot ſeaſon, nor did I take ſufficient care of the bottles; hence, out of

of many bottles that I ſealed after charging them, one only could afford ſome remains of electricity, and thoſe very ſmall, two days after. This very ſame fact, though it rather confirmed me in my opinion, (I intirely imputed my want of ſucceſs, to my want of ſkill in making the experiments) made me fear that the imperfection of the experiment would rather make the caſe worſe with cavillers, than throw a light on the ſubject: but Mr. Canton, as if he had intended to relieve me from my fears, wrote to Dr. Franklin the 31ſt of October, 1760, in the following terms. "Having procured ſome thin glaſs balls, about an inch and an half in diameter, with ſtems or tubes of eight or nine inches in length, I electrified them, ſome poſitively on the inſide, and others negatively, after the manner of charging the Leyden bottles, and ſealed them hermetically. Soon after I applied the naked balls to my electrometer, and could not diſcover in them the leaſt ſign of their being electrical: but holding them before the fire, at the diſtance of ſix or eight inches, they became ſtrongly electric in a very ſhort time, and more ſo when they were cooling. Theſe balls will, every time they are heated, give the electric fire to, or take it from, other bodies, according to the *plus* or *minus* ſtate of it within them. Heating them frequently, I find, will ſenſibly diminiſh their power; keeping one of them under water a week, did not appear in the leaſt to impair it. That which I kept under water, was charged on the 22d of September laſt, was heated ſeveral times before, was kept in water, and has been heated frequently ſince; yet it ſtill retains its virtue in a conſiderable degree. The breaking two of my balls accidentally, gave me an opportunity of meaſuring their thickneſs; which I found to be between ſeven and eight parts in a thouſand, of an inch. (The reader may ſee the complete edition of the works of Dr. Franklin, publiſhed in London in 1769, p. 401).

311. In conſequence of this diſcovery of Mr. Canton, Dr. Prieſtley, the 30th of December, 1766, "examined a tube three feet in length, one half of which he had charged in the month of March preceding, and then ſealed hermetically; but could not perceive that it was excited in the leaſt degree, either by heating or cooling. The difference in the reſult of this experiment from

from ſeveral of Mr. Canton's, related p. 296, he attributed to the thickneſs of the glaſs tube. Mr. Canton charged ſmall balls exceedingly thin. He alſo obſerved that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceeding well. He afterwards opened this tube, and pouring a quantity of leaden ſhot into it, found it to contain a very good charge. It gave him one conſiderable ſhock, and ſeveral ſmall ones; as he made no uſe of an outward coating, but only diſcharged it by graſping it in ſeveral places by his hand."

312. From theſe experiments we may conclude it as a certain truth; I. that glaſs, when it is not dilated by a certain degree of heat, does not conduct the electric fire. II. That glaſs receives a charge, or, in other words, contracts equal contrary electricities on its oppoſed ſurfaces, only ſo far as it is not traverſed by the electric fire. III. Therefore, the action by which an electricity raiſed on one of the two ſurfaces, raiſes an electricity contrary to it, on the oppoſed ſurface, is not an *immediate* action, but is produced within the ſubſtance of the glaſs, by ſome vibration or preſſure, or removal of ſome *ſupport*, or in general, by ſome alteration or other, produced by the exciting electricity, either in the glaſs, or the fire inherent in it. IV. Laſtly, the equality between two contrary electricities that conſtitute the charge, ceaſes to be permanent, whenever the glaſs begins to be penetrated by the electric fire. Hence, though when Mr. Canton heats his balls of glaſs, a deficiency at firſt takes place on the outward ſurface of them, equal to the exceſs raiſed in the inſide, yet, this exceſſive fire will no ſooner be able to paſs from the inward to the outward ſurface of the ball, than it will remain without any effect; or at leaſt it will only have that of filling up the outward deficiency, and no electricity will of courſe be manifeſted: therefore we may ſay, that in proportion as the heat begins to dilate the glaſs, the outward ſurface of it draws nearer to its natural ſtate; alſo, that the inward exceſs paſſing through the glaſs, and at laſt accumulating itſelf on the outward ſurface, manifeſts itſelf there by the uſual ſigns. In fact, that the glaſs once rendered penetrable, does not require any longer to poſſeſs the two contrary electricities, in order

order to its being able to manifeft either of them, plainly refults from what I faid in my book of the *vindicating* electricity, p. 63, 64. I warm a plate fo intenfely, that I cannot hold it for a fingle moment, I prefent it to a rod that hangs from the chain, and it becomes electrified by excefs in every one of its parts, juft as if it was a body that were *deferent* by itfelf. V. It follows moreover, that if, in order to preferve the charges, Mr. Canton has been obliged to keep them even from the heat of an autumnal day, we may conclude, that the electric fire can be enabled to pafs through glafs, (though perhaps very fcarcely and flowly) by a very inconfiderable degree of heat.

313. To proceed now to the object of this chapter, I obferve that the glafs, when thoroughly impervious to the electric fire, abfolutely prevents difcharges from taking place; and that air, being always more or lefs impregnated with deferent particles, and befides of a very moveable nature, can only retard them. I have many times found a remnant of charge in bottles, fixteen hours after charging them; I found them fo, fometimes, after twenty-four hours, or even more; and from my experiments, as well as from thofe of others, it refults, that the duration of fuch remnants is always in proportion to the drynefs and purity of the air: fuch proportion again proves, that the fubftance itfelf of the glafs, has not of itfelf the propriety of transfufing and diffipating charges.

314. The queftion has been propofed, whether, in order to preferve the charge of a bottle, it muft be kept on an infulating or a deferent body. Certainly, if we touch the hook of it, and that the body on which it is laid, be deferent, a difcharge will enfue; becaufe the electricity of the hook will diffipate itfelf through the perfon who touches it, and at the very fame inftant the other electricity will alfo diffipate itfelf, through the deferent body, into the ground: but in order to refolve the queftion, whether the electricity be better preferved by placing the bottle fo that either, or both, its furfaces be infulated, I have made the following experiment. I hold the belly of the two bottles, A B, *a b* (Pl. II. fig. 6.) and prefent at once their hooks to the chain, and when the electrofcope informs me it is proper time to do it, I take them off

off at once. They being thus charged, I place the one on the ground, the other on the opening of a tumbler or glaſs, which is very dry, and likewiſe placed on the ground: and after whatever intervals of time I take up theſe two bottles, whether it be of one, two or more minutes, I cannot perceive the leaſt ſpark take place between their hooks, which I all the while preſent to each other in the dark. Therefore, I conclude that theſe bottles which are equal and ſimilar to each other, have both of them a long neck (which laſt circumſtance muſt be obſerved), and are equally charged, loſe equal portions of their electricities in equal intervals of time, though the one be inſulated in both its ſurfaces, and the other in that ſurface only on which the electricity has been immediately raiſed.

315. Let us now examine the act itſelf of the diſcharge, and the circumſtances that contribute to weaken it, the quantity of charge being ſuppoſed to be known. Such weakening of the diſcharge may happen in two different ways; either becauſe the iſſuing of the ſpark is *lengthened*, or part of the ſame is left behind; and the diſcharge is the greateſt poſſible, only when the ruſhing of the ſpark is both *ſimultaneous*, and conſiſts of the greateſt portion poſſible of the exceſſive fire. Theſe circumſtances may be obſtructed by all the ſeveral kinds of reſiſtance which the fire may meet with, either in gathering itſelf from various points of the redundant ſurface towards that place to which the conducting bow is preſented; or in getting into the conducting bow itſelf, or alſo in paſſing through the ſame; or even in diffuſing itſelf from the ſame, to the different points on the deficient ſurface of the glaſs which is to receive it.

316. Therefore, coatings deferent to a high degree, as I obſerved before, perfectly continuous, and of a ſufficient thickneſs, will cauſe the fire moſt readily to run to that place whence the diſcharge is excited, and moſt rapidly to diffuſe itſelf over the deficient ſurface. I touch ſeveral conductors with my finger, which I inſtantly remove: if the conductor be perfectly deferent, for inſtance of braſs, no farther ſpark can be drawn from it; but if the ſame is made with gilded paſteboard, I can then draw a ſecond ſmall ſpark from it, which is proportioned to the very ſmall, it is true, but yet very frequent interruptions in the gilding. If the

faid conductor be made of pafteboard alone, I then can draw a number of fparks proportioned to the much greater refiftance of the naked pafteboard, however damp it may be. I repeat it therefore, that metallic coatings perfectly continuous, will contribute much to unite the difcharge, will leffen any remnant of it, and confequently ftrengthen it; I therefore prefer coating my glaffes with fheets of tin or lead, rather than to ufe water, which is deferent in a lefs degree; and I prefer fuch continuous coatings to the ufe of fmall fhot of lead, or even filings of iron, becaufe the latter do not form fo continuous a body: iron filings are befides liable to grow rufty. When the bottles are but fmall, I fill them with mercury, which of itfelf is extremely deferent, and touches in all parts the furface of the glafs.

317. In the fecond place, befides, were we minutely to defcend into every particular, we might add, that the fpark will probably be ftill more united and fimultaneous, if the heads of the conducting-bow are applied to the centre of the coatings: we fhall fee with refpect to this, that a certain *law* obtains concerning the direction of the fpark, which will confirm this fufpicion; the only confideration we fhall offer here to the reader is that of the following mechanic principle, viz. that an elaftic fluid will beft gather itfelf into that point, to which it may run from equal diftances, on all fides.

318. Certainly, a fufficient capacity and continuity in the conducting bow muft alfo contribute to facilitate the uniting together the whole charge. Thus a very ftrong difcharge will indeed be effected through a ftrip of gilded paper two or three inches long; but if the fame be extended to the length of fome feet, the fmall refiftances from the fmall interruptions in it will be multiplied; the difcharge will produce but a fmall crack, and will only be completed, for a great part of it, by a continued rattling. Commonly, fmall Chains are ufed to carry the difcharge, and as every ring of it only touches that next to it in a few points, thence reresults a degree of refiftance proportioned to the fmallnefs of thofe points. The number of fuch refiftances is indeed infinitely lefs than thefe from the interruptions in the gilt paper; no great difference, when a chain is ufed, will refult either in the crack, or the other effects of the fpark, at leaft as far as the bluntnefs of our fenfes

will

will allow us to judge; but yet, ſuch chain as we ſhall better ſee hereafter, affords ſome degree of reſiſtance, therefore I prefer a continued rod of braſs, a fourth of an inch thick, and I obſerve not to anoint with oil the ſcrews into which terminate the two ends of it, which are deſtined to be inſerted into the balls M O; becauſe ſuch ointment would produce ſome reſiſtance; and indeed I have ſometimes obſerved a portion of the ſpark externally to lighten from that line which parts the rod from the ball.

319. But a farther reſiſtance can alſo ariſe from the conducting bow itſelf, whatever may be its capacity and intrinſick *deferent* quality; with regard to this, a more minute diſcuſſion is, I think, neceſſary. The experiment of the bottle of Leyden was ſcarcely known, when the Abbé Nollet and Mr. Monnier, junior, undertook to diſcharge ſuch a bottle, through long conductors, either to try to what diſtance ſuch diſcharge might be carried, or to diſcover the velocity of its paſſage. I ſuſpect, that in that experiment eſpecially in which they attempted to diſcharge the bottle through an iron wire a league long, the diſcharge took a much ſhorter way, ſince the wire lay on the green graſs, went over wooden fences, and was afterwards ſpread over a newly tilled ground. We have obſerved before, that in order to complete a diſcharge, there is no neceſſity that the very ſame fire which runs from the redundant part, ſhould itſelf reach the defective ſurface; we ſhall ſoon have occaſion to obſerve, that electrical fire diffuſes itſelf into that receptacle which lie neareſt at hand, though it may be of a nature ſomewhat leſs deferent, and that it alſo runs from the neareſt place that can ſupply it.

320. In the courſe of their experiment, the Engliſh philoſophers made the ſame obſervation, and obviated the error to which they would elſe have been expoſed, by inſulating their long iron wire on ſticks of wood dryed in the oven. I ſhall relate the laſt and greateſt experiment that was made relative to that object under the direction of Mr. Watſon. They inſulated an iron wire, which, by the numbers of its windings, completed the ſpace of two miles; the electric Machine was in the middle of the circuit, and there an obſervator held in each of his hands one of the two wires, each a mile long; the other end of one of the ſaid wires communicated with the outſide of the bottle, and the other end of the other wire communicated with the Chain, to which the bottle was ſuſpended. The diſcharge of

the bottle was repeated feveral times, and not the leaft difference in point of time could be perceived between the explofion and fhock, by the Obfervator who united in both his hands the two wires, each a mile long.

321. But what is more effential to our prefent object, is, that in all the explofions thus made through conductors of a very confiderable length, it was obferved, that though the bottle was completely charged, yet the crack from the gun-barrel that ferved as a prime conductor, was lefs ftrong than when the difcharge was effected in a room; fo that, fays Dr. Watfon, an obfervator converfant with the fubject would not have imagined, from the fmall fpark and noife that took place, that the ftroke at the extremity of the wire was in any degree confiderable. (See Prieftley, p. 207.)

322. Before I had read fuch obfervation, I had already perceived fomething like it in making an experiment which we have occafion to repeat feveral times in the year in the rooms where our Univerfity's machines are kept. In order to make this experiment I pound fome gunpowder into a very fine powder or duft, and ram it into a ftrong tube of glafs, fo as to fill up all interftices within it; this done, I fix into it two brafs rods A B, (Pl. VI. fig. 9.) fharpened at their ends, and place them at fuch a diftance from one another, that the fpark may leap from the one into the other; I afterwards place the tube thus difpofed, within the circle of the difcharge of my ordinary electric fquare, the coating of which is not quite three fquare feet, and then I obferve the following particulars. I. When the circle of the difcharge is fhort, the crack and the light produced by the fpark, are extremely intenfe, and the latter kindles the gunpowder, as it paffes from one of the above points to the other, though diftant from each other a quarter of an inch and more. II. But if I make the circle or compafs of the difcharge much longer (I feldom make it longer than one hundred feet) then, though I may infulate either, or both, of the brafs wires that conftitute the communication, no kindling of the gunpowder will enfue. III. In order to kindle, it is neceffary that the two points fhould be advanced to an eighth part of an inch from each other. At the fame time, the fpark does not leap from the electric fquare but later, that is to fay, only when that

that end of the wire which is to draw it, is prefented to the coating at a diftance lefs than ufual; it is thrown alfo with lefs force, and with a much lefs crack; the light as well as the crack are not fimultaneous, but have in them a kind of *fucceffion*, which our fenfes may, with fome degree of attention, diftinguifh, though the time cannot, I think, admit any kind of menfuration.

323. This experiment ferves to confute that dangerous paradox, viz. that a fpark moft freely circulates through different bodies, without fuffering any obftruction from their extent, whatever it may be. It alfo demonftrates that metals, in whatever degree they may be more deferent than other bodies, yet afford fome refiftance to electric fparks, which refiftance is proportioned to the length of the paffage, which thofe fparks are to take through them, (fuch refiftance in my brafs wire could not proceed from its want of capacity, it being fomewhat above a twelfth of an inch in diameter) that is to fay, proportioned to the length of the column or body of electric fire which lies diffufed within the metal, and which the fpark, when it gets into the latter, muft drive forwards: juft as Air, when I blow within a tube, proceeds no otherwife but by driving forwards the air formerly contained in the fame. Therefore, the motion of the electric fire is fubject to the common mechanick laws of *action* and *re-action*, from which it feemed at firft moft extraordinarily to deviate. And as a laft confequence, I fhall obferve, that the above experiment, and that of the light we have called *overflowing light* (142.), reciprocally affift and explain each other, as well as confirm the theory.

324. From thefe facts an important truth moreover follows, which is, that the deference in bodies muft not, for the future, be eftimated only from the degree to which they of themfelves poffefs fuch property, and from their electric *capacity*; but muft alfo be looked upon as being proportioned to the length of thofe bodies, inverfely. Certainly, thofe brafs wires which I ufed, being confidered in themfelves, are better conductors than my own body, yet, when I join them together into a fingle wire, fo that they conftitute a conducting bow two hundred feet long, and try, with fuch bow, to affect a difcharge, part of the fire leaves the bow or wires, and fairly runs through my body, in confequence of the

 greater

greater refiftance than ufual it meets with in moving through that ample and moft deferent, it is true, but alfo very extenfive metallic wire.

325. From the above obfervations, I have farther underftood the reafon of a fact, which indeed ought originally to have fuggefted to me this prefent theory of the *refiftance* proportioned to the *length* of the paffage, but which I had poftponed to mention, till I had a few others more decifive to confirm it. After charging fmall plates of glafs, I fometimes happen to difcharge them by touching the coating of the one furface with my thumb, and the coating of the other with the index of the fame hand; and in this cafe I find, that my two fingers are ftruck much more ftrongly than when I difcharge the fame plate by touching the two coatings with my two hands; I fhall moreover add this, that in the former cafe, the fpark is alfo ftronger and more fimultaneous in proportion with the fhorter way it has to go through.

326. In fhort, we have this demonftrated, viz. that all other things being equal, the fhorteft paffage procures the greateft difcharge, but feveral accidental circumftances in the body that affords fuch paffage contributes alfo either to weaken or ftrengthen the difcharge. Among fuch circumftances we muft not certainly forget to mention the fhape and dimenfions of the two ends of the conducting bow, which is to form the communication. If fuch ends be fharpened, the difcharge, if confiderable, will not indeed be effected filently, notwithftanding it fometimes happens, that the copious fire contained in clouds, is filently drawn by fharp metallic rods; no, this will not be the cafe here, unlefs the end of the conducting bow be prefented very flowly, and if the charge be extremely weak, a fpark fufficiently copious will indeed be thrown; but if fuch fpark be attentively examined, it will be found to be much divided, the fharp point beginning to draw fire before it reaches the ufual diftance, and the difcharge will in confequence be lefs ftrong than when the fpark is drawn by a bow terminated at both its ends by balls exactly round, which can draw the fire at once. With refpect to this however, fome limitations muft be obferved, becaufe, if the diameter of the balls be two great, another kind of divifion in the fpark will take place, which will equally weaken the effect of it: if, for inftance, a given fpark, which, in leaping

into the ball of my conducting bow, which is of half an inch in diameter, keeps united into a ſpace of one twelfth of an inch, leaping now into a ball of a diameter double to that of the former, it will perhaps ſpread itſelf on its ſurface over a ſpace double to that occupied in the firſt caſe; all which will be more fully proved in the fourth Section, where I ſhall ſpeak more explicitly of the effects and accidents relative to ſparks.

327. But laſtly, the *reſiſting* medium itſelf, through which a portion of the ſpark is to ruſh, may be ſo prepared beforehand, as to tranſmit it more united, more loud, and conſequently more effectual. I. Thus I excite the ſpark through a hole a twelfth or a ſixth of an inch wide, bored in a ſmall plate of glaſs, or of talc; in fact, every body may have obſerved, that when he preſents his finger to the Chain, or to the Machine, when animated by a ſtrong electricity, the fire begins to rattle at a greater diſtance than that at which ſparks are thrown; and thus the ſubſtance deſtined to form the ſpark is divided, and the latter of courſe weakened. Now, a circumſtance exactly like this takes place, when the conducting bow is preſented to the coating of a charged glaſs, eſpecially if ſuch coating be not even and poliſhed; in ſuch a caſe, a portion of the charge, if ſomewhat intenſe, runs before the reſt, partly ſilently, partly with cracks, and that ſpark which follows is proportionally weakened; but if the coating be guarded by an inſulating plate, the fire that gathers at the ſmall hole remains united, counteracts and balances itſelf, as we ſhall ſee in the next Section, and ruſhes to the conducting-bow, much more compact and rapid.

328. But I moreover uſe another contrivance in order ſtill more to unite the paſſage of the ſpark into the ſame point of time; this contrivance is deduced from my principle, *that the ſpark drives deferent bodies into its own way*, as well as from the experiments I made in the ſixth chapter of the *Artificial Electricity*, where I proved that ſparks drive water into a moſt active vapour. I reaſoned thus: a few particles of water driven into vapour will occupy a ſpace many thouſand times greater than before, and will conſequently moſt rapidly exclude the air from the ſame ſpace; therefore, if the principle of the ſpark (we ſhall ſee hereafter what is to be underſtood by the *principle of the ſpark)* really drives water into vapour, and

if,

if, according to the above law, it drives it into that way which it marks out for the ſpark; it will thus open a very free paſſage to the body itſelf of it, which is to follow, ſo that the whole fire will ruſh through ſuch paſſage moſt compact and rapid. In conſequence therefore, of ſuch reaſoning, I wet a little, either with ſpittle or water, that portion of the glaſs which lies around the hole through which I propoſe to draw the ſpark, and thence always follows a moſt loud crack. In ſhort, I help art with the ſame means which, as I ſaid in another place, (Terreſtial Atmoſ. Elec. pag. 250.) Nature herſelf makes uſe of when ſhe diſplodes her loud, formidable arrows.

329. Thus, as in the preceding chapter, we have learned how to produce the moſt intenſe charges, ſo in this we have diſcovered how to effect the moſt intenſe and effectual diſcharges. To the latter purpoſe, a conducting bow muſt be uſed, in its nature extremely *deferent*, of a ſufficient thickneſs, and of the leaſt length poſſible. The ſame muſt be ſupplied with knobs at both its ends, well poliſhed, and of a proper ſize; the ſpark muſt be excited through a hole ſufficiently wide, bored through an inſulating body; and the ſpark muſt be as it were aſſiſted by a drop of water, through which it may open a free paſſage to itſelf, acroſs the reſiſting medium. Laſtly, while the ſpark is excited, the electricity of the ſyſtems muſt continue to be animated, that any accidental diſſipation of the fire may thus be inceſſantly repaired.

CHAP. VII.

In which ſome queſtions are reſolved, which ſerve ſtill more to confirm the theory on the charges and diſcharges of inſulating bodies.

330. FIRST queſtion: *Do not the coatings, beſides their reſpectively diſtributing and uniting charges, ſomewhat contribute towards increaſing the intenſity of the ſame?*

331. Dr. Franklin took off the hook and lid of a charged bottle A, and decanted the water in it, into the bottle B, obſerving in the mean while to have both bottles inſulated; then placing again the hook into the bottle A, he received nevertheleſs, a ſhock from it, while he obtained none from the bottle B, into which he had decanted

decanted the water. Perhaps might it be fufpected, that the *ftriking* electricity had remained behind within the fur of moifture which continued to coat the infide of the bottle A; but another experiment made by the fame gentleman removes this ground of fufpicion; he ftripped a charged plate of its metallic coating, and he was neverthelefs ftruck by it. But all objections whatever, on this head, are farther confuted by the experiment I have fince made: I charge a naked plate by the means of the brufh and ftar on both its furfaces; and after fupplying the fame with metallic coatings, which are moreover infulated, I receive a fhock: this therefore remains afcertained, viz. that the electricities which conftitute the charge, do not refide within the coatings.

332. *The coatings do not increafe the intenfity of the charges and difcharges, but as far as they receive a certain quantity of electricity on that of their furfaces which remains uncovered; which quantity is exceedingly fmall in comparifon to the electricities that refide in the oppofite furfaces of the infulating body, which conftitute the charge.* In the following chapter I fhall prove, that the electricity of *deferent* bodies intirely refides on their furface, and in no fhape whatfoever within their fubftance; but independently of any farther proof we may give hereafter on this head, the long feries of fparks that is requifite to difcharge an infulating body *by alternation*, (No. 290.) is alone fufficient fully to prove, that each of the fparks drawn in that kind of experiment, expreffes the whole electric *capacity* of the coating; whereas it is the fum itfelf of all thofe fparks which expreffes the quantity of the fire that conftitutes the charge.

333. *Nor is fuch electricity in the coatings, anywife neceffary to promote the electricity that conftitutes the charges.* Let us take a bottle full of water into which is dipped a fharp point of iron; there is no electricity within the water that ferves as an interior coating to the bottle, and when it comes to be prefented to the Chain, or to the rubbed glafs itfelf, it immediately takes its charge; yet, the whole capacity of fuch coating, is reduced to the extremely fmall capacity of that point of iron.

334. I have covered with fealing-wax ground into very fine powder, the fuperior furface of the plate of glafs A B. (Pl. vi. fig. 8.) and heating the fame very flowly, brought it to flow, fo

that a ſheet of tin might be fixed to it. I again ſpread powdered ſealing-wax on this coating, and after heating it, I applied to it another plate of glaſs perforated in G, and alſo very hot; ſo that the two plates with the coating thus in the middle, remained very firmly united to each other. I have, proceeding after the ſame manner, annexed another coating to the other ſide of the plate A B, and on it another plate of glaſs E F, perforated in H, ſo that the whole formed a maſs compoſed of a plate of glaſs A B, with its two coatings covered with wax in the middle, and of two other plates C D, E F on each ſide of it. Laſtly, with a ſharp knife I uncovered all that part of the inward plate A B, which correſponded to the holes G H; I filled the vacancy with ſhavings of tin, and through ſuch communication I tried to introduce a charge into the inward plate A B. This done, I diſcharged it; when I received ſuch a ſhock as I never experienced before from a common plate of an equal ſize to this. Now, in ſuch apparatus the ſealed coatings have of themſelves no electric *capacity*, except indeed, that of the very ſmall ſurfaces of the ſhavings of tin-ſheet, inſerted into the holes H, G.

335. Whoever has a ſufficient degree of patience, will, by the ſame means, procure an electric battery moſt commodious and effectual. Let twelve plates be paſted together in the above manner, with their coatings in the middle between them, and let pieces of the coatings of the plates hang out on each ſide alternately; then let theſe twelve plates be again faſtened with ſealing-wax, between two plates of glaſs externally naked. In ſuch ſtate of things, the Chain will ſend fire into thoſe plates which are contiguous to the metallic ſtripes which get out of the general maſs, with which it communicates; and mean while the Machine will draw the fire from thoſe plates which are contiguous to the metallic ſtripes with which it communicates. The contrary electricities which will take place within the whole, cannot poſſibly intermix with each other; no electricity beſides, will diffuſe and loſe itſelf through the margins, ſince none are left. Laſtly, the total weight of this electric battery will be the leaſt poſſible, if we conſider its capacity: it will alſo occupy the leaſt poſſible ſpace, ſince under the volume of leſs than half a cubic foot, will be comprehended (if each plate admits

of

of a coating of one ſquare foot) a battery equal to twelve ſquare feet.

336. Second Queſtion. *To what muſt be attributed the remnants left by diſcharges?*

337. *The principal part of ſuch remnants is proportioned to the imperfection of the communication, and is proportioned to it.* When I diſcharge the electric ſquare A B C D, (Pl. II. fig. 8.) I bring my conducting bow into an immediate contact with the oppoſite coatings; the greateſt poſſible ſpark is thus thrown out; and then if I inſtantly take off the bow, but afterwards touch again the ſquare with it, I only obtain a very faint light; if on the contrary, I do not touch the ſquare, but diſcharge it by keeping the knob of the bow at the uſual diſtance through which ſparks ruſh, then, when I touch the ſquare again, I draw the remaining fire which has not been able to leap through the reſiſting medium, air. If I direct the ſpark through a ſubſtance that affords a certain reſiſtance, for inſtance, though a ſtripe of tin-foil, extremely narrow, and conſequently of a very ſmall capacity, or through metallic *calces*, which are deferent but in a ſmall degree; in theſe caſes alſo, when after the diſcharge is made I touch again the ſquare, I find a remnant of a ſpark proportioned to ſuch increaſed reſiſtances; and ſuch a remnant may, by increaſing the reſiſtance, be increaſed at pleaſure, and the diſcharge be diminiſhed accordingly. A diſtinction however, muſt be made with regard to theſe kinds of reſiſtance which as ſoon as they begin to be ſurmounted, inſtantly afford a moſt free and open paſſage to the diſcharge: thus a diſcharge tranſmitted through water, which of itſelf is not a ſubſtance deferent to the higheſt degree, yet leaves a remnant of diſcharge exceedingly ſmall; the reaſon of this is, as has been explained before, that water, when driven into vapour, affords a moſt free paſſage.

338. But even after a perfect communication has been eſtabliſhed between the two oppoſed ſurfaces, there is ſtill a remnant of a diſcharge which is more difficultly drawn, which ſeems to adhere to the glaſs, and after ſucceſſive intervals of time, affords a ſeries of other ſparks, whence therefore does ſuch a remnant proceed, which I may call a *reluctant* remnant?

339. This remnant, I doubt not, proceeds for the greateſt part, from the redundant electricity, which, in the act of charging, is driven beyond the coatings, into the naked margins of the glaſs. Such electricity is perceived, either while it actually accumulates itſelf on the margins, or in preſenting threads to theſe ſame margins, which are immediately attracted by them; other circumſtances may beſides contribute to increaſe this electricity, ſuch are, I. The ſtrength with which the charge is effected. II. The thinneſs of the glaſs. III. The extenſion of one of the coatings beyond the correſponding limits of the other.

340. In glaſſes to which their coatings have been paſted with a ſubſtance that is neither inſulating nor perfectly deferent, it may be, that a part of thoſe *reluctant* remnants proceeds from the reſiſtance which the diſcharge neceſſarily meets within ſuch ſubſtance. In an electric ſquare to which its coatings were faſtened with paſte made with ſtarch, the ſaid coatings were, after a matter of a year, for the greateſt part ſeparated. I do not know whether this aroſe from the force of the diſcharges; but ſuch ſeparation leſſened much their intenſity, and it has been rendered the ſame only when I have again faſtened the coatings to the plate.

341. In the abovementioned apparatus of a plate of glaſs coated by the means of ſealing-wax, the metallic coatings are immediately applied on the ſtratum of ſealing-wax, which, together with the plate, conſtitute a ſingle inſulating body; therefore, no reluctinq remnant can ariſe from any reſiſtance of the paſte that has been uſed; and that which may ariſe from an electricity guſhing out around the holes above mentioned, may eaſily be ſuppreſſed. I therefore proceeded thus: when I had charged jointly both the waxed plate A B, and another plate of the ſame ſize that ſtood by itſelf, and was coated after the uſual manner, I placed both on a deferent table; I then ſpread a metallic coating on the outward plate, and the whole maſs was thereby diſcharged. This done, I immediately examined whether there was a remnant of electricity in the coating I had thus placed on the plate C D, by preſenting a very thin thread to it, and I always found ſome ſigns of electricity, it is true, but they were exceedingly weak, and of an extremely ſhort duration, ſo that the thread only moved a few times.

times. I then difcharged the plate that was coated in the ufual way, and examined the ftate of its coating; when I found it to continue for a very long while to draw the thread with a confiderable degree of force; fo that the electricity which remained on the coating that had been applied to the former plate, might be called nothing when compared to that in the latter. I made this experiment in winter time.

342. I was therefore by no means induced to conclude, that any confiderable part of the *reluctant* remnant proceeds from a quantity of electricity which penetrates into the fubftance of the plate of glafs, to fuch a depth as to make it difficult to extract it from thence (which was the object I chiefly propofed myfelf to inquire into); and the more fo as even the very fmall remnant which I perceived in the plate C D, might very well proceed from fome imperfection in the contact between it and the metallic fheet, which perhaps I vainly endeavoured perfectly to fpread upon it, though I took every poffible precaution to make the contact perfect.

343. To conclude; the greateft portion of reluctant remnant in a plate of glafs, fupplied with coatings exactly fhaped and completely deferent, muft be attributed to redundant electricities, which by the force of the charge, are driven to a fomewhat confiderable diftance beyond the coatings, and are ftronger in proportion as fuch diftance is lefs. When the charge is fuppreffed, fuch electricities gradually diffipate; one part of them gets again into the coatings; the neareft make their way firft, and afford the firft fparks which are ftronger; to thefe gradually fucceed weaker ones.

344. Third Queftion. *Whence proceed the holes and fiffures perceived in glaffes, and what kind of information can we draw from them, with regard to charges and difcharges?*

345. I blow, (as I obferved before in Numb. 468 of *Artif. Elec.)* two thin balls of glafs, and fill them with mercury, water, or filings of iron, and infert into their necks one end M of the conducting bow M N O, (Pl. II. fig. 8.) and with the bow thus armed with glafs, I try to difcharge the fquare A B C D, when I find that a fpark is thrown from it, that bores through the ball, a round hole, which is about one-twelfth part of an inch in diameter, without

without any other fiſſure: this is what pretty commonly happens when the balls are made of an excellent glaſs, without bubbles or other imperfections, and every where equally thin.

346. But in glaſs leſs perfect, or even in thoſe that are perfect when the operation is attempted after a different manner, many accidents take place, of which I ſhall only mention the moſt remarkable. I. When there are bubbles in the ſubſtance of the glaſs, it ſeems that the ſpark marks out and chooſes thoſe that will reſiſt leaſt; nor does it always perforate the whole of ſuch bubble, but ſometimes only bores a little hole, through which the point of a ſmall pin hardly could paſs; ſometimes a freſh hole is made in the centre, ſometimes in the ſide, ſometimes, where two bubbles join: I commonly diſcover ſuch holes, by blowing into the ball and receiving the wind on my hand. II. It often happens, that from the hole, whether it be narrow or wide, ſeveral fiſſures run, which frequently unite into one, ſo that pretty large pieces fall from the bottom of the ball, which makes it difficult to find out the original hole; even ſometimes the whole bottom is beaten into the ball. III. At times, the hole happens not to be completely round, and then it is of an oblong or angular form where the circle ceaſes. This however muſt be obſerved, that that hole which is the immediate effect of the ſpark, has in it very diſtinct characteriſtics from thoſe which ſimply reſult from accidents in the fiſſures: the edges of the latter are poliſhed and bright, as is the caſe in all common fractures of glaſs; but the edge of the hole made by the ſpark, is covered with furrows converging towards the inſide of the ball; which make it look rough and whitiſh, as is the caſe with glaſs reduced into a fine powder. IV. When I put in the inſide of the ball, neither water, nor filings of iron, but ſimply inſert into it the end of the conducting bow terminated into a point, then the furrows on the edge of the hole converge towards a nearer point within the ball, and the hole grows much more contracted from the outſide towards the inſide of the ball: all which is conformable to the nature of the ſpark itſelf, which grows contracted in proportion as the compaſs on which it is to act, grows narrower.

347. But

347. But of all ſuch holes, thoſe which afford moſt information are, thoſe which are leaſt perfect, that is to ſay, thoſe in which the portions of glaſs rent by the ſpark, partly or all of them remain in their reſpective places. It is not a rare caſe that a number of them remain unmoved; but that all ſhould remain is a very rare thing, and which out of a great number of attempts has only ſucceeded to me once or twice; I ſhall give a deſcription of the moſt remarkable caſe I have obſerved. I. Exactly from the ſame viſible point on a ball of glaſs of a middling quality, (I had put filings of iron into the ball) ſeven fiſſures ran, five of which were extended to two-thirds of an inch, from the point itſelf; the two others went to the diſtance of an inch and more; and the ſector contained between the longer fiſſures was raiſed ſomewhat above the ſectors contiguous to it; whence I gueſſed that the ſtroke given by the ſpark had rapidly dilated the glaſs, and rent it ſo as to raiſe the ſectors included between the fiſſures, the largeſt of which had been prevented from falling back by its own aſperities, and by thoſe of the ſectors contiguous to it. II. But the moſt remarkable circumſtance was the opaque whitiſh colour obſervable on the top of the ſector around the point from which the fiſſures ſprung; the place thus rendered opaque, formed a circular band a tenth of an inch broad, of which the ſaid point was as the centre; this band being obſerved with a microſcope appeared as if it were divided into a circle more opaque than the reſt, then into another circle a little more tranſparent, into another ſurrounding the latter, which was compoſed of ſeveral white points, and into a number of others of a changeable colour. The edge of the fiſſures within the extent of the white circle was likewiſe white; but examining it with a lens the focus of which was ſix lines diſtant, the whiteneſs ceaſed to be continuous, and was divided into a number of ſhining-coloured particles; the ſector that had remained raiſed above the others afforded me an opportunity of examining its edge.

348. Now, the reſult of all theſe obſervations appears to me to be this; I. The hole could not be produced by a ſimple mechanical ſtroke of the ſpark on the ſurface of the ball, but all the abovementioned accidents demonſtrate both an internal action, and a penetration. II. My opinion is, that in order to produce all the above

accidents,

accidents, the ſpark muſt have vehemently ſhaken and driven forwards the electric fire inherent in the ſubſtance of the glaſs, and have followed it with its whole ſtream, dilating the minuteſt interſtices within the ſubſtance of the glaſs, and dividing its particles ſo as to leave them adherent to each other only in ſome few points; whence aroſe the whitiſh colour of the glaſs, exactly like that aſſumed by it when powdered very fine. III. I think moreover, that in conſequence of this penetration and paſſage of the ſpark, ſuch a dilatation muſt have ariſen by the force of the penetrating and heating element, as to have broken the glaſs in the ſhape of ſeven rays, and raiſed the largeſt ſector above the others.

349. Beſides the whitiſh colour of the lucid, or coloured particles, both the whiteneſs of the fiſſures which ran between the lucid or coloured parts, and the lucidity or colour of the abovementioned points, prove, that all theſe accidents aroſe from mere ſeparations of parts; the one very minute, the other ſomewhat more continuous. I did not forget to examine whether on that ſpot where the white colour obtained, there was any ſign of a mixture of a matter rather calcinable than vitreſcible, and could not find the leaſt indice; the ſurface was extremely ſmooth, without the leaſt appearance of any rugoſities, and the whitiſh colour loſt itſelf into the contiguous tranſparent glaſs, not at once, but gradually. Laſtly, when I again obſerved the fiſſures, either by the common light of the day, or with a microſcope well illuminated, I found that the white appearances on the exterior edges intirely vaniſhed; they were changed into a number of very ſharp bright points, and theſe edges beſides appeared as if compoſed of thin ſtrata, the one more, the other leſs, extenſively broken.

350. With regard to the circular ring around the lucid points, I ſhall defer to ſpeak of them till I ſpeak of the ſimilar phenomena, which have alſo been obſerved by Dr. Prieſtley, on the ſurface of metals; I ſhall only ſay here, by the way, that ſuch points proceed from ſuch portions of the ſpark as are ſcattered around the central and main body of it.

351. But, to proceed at length to the queſtion which I at firſt propoſed to reſolve; from the above obſervations I ſtrongly conjecture, that *the charges and diſcharges of glaſſes are produced by an*

action

6

action which is propagated from the one ſurface of the glaſs to the other, not ſo much by the means of any vibration of the glaſs itſelf, as by the means of a vibration, or commotion, or modification, whatever they may be, of the electric fire inherent in the glaſs. In fact, if ſuch a vibration or commotion as we mention, leaves behind it thoſe viſible marks we have juſt deſcribed, when a very ſtrong charge is communicated at once by *diſtribution*, (when the balls of glaſs receive the ſtroke, they become very ſtrongly charged by *diſtribution*, as much as the nakedneſs of the one or both of their ſurfaces will admit,) it is moſt likely that a vibration of the ſame kind, though leſs, will ſerve to produce ſome degree of charge.

352. This would alſo obviate another difficulty of ſome importance, which is, how bodies ſo different from each other as glaſs, porcelain, ſulphur, roſin, &c. take a charge in the ſame manner: it is enough for this, that the element inherent in them be the ſame, and poſſeſs in all the ſame aptitude to receive ſimilar vibrations.

353. But when I afterwards attentively conſidered the experiment which I had already deſcribed in Numb. 302 of *Atmoſ. Terr. Elect.* my ſuſpicion turned into a certainty. The experiment I mean conſiſts in tranſmitting a ſtrong ſpark through a deferent body of a ſcanty capacity, for inſtance, through a ſtripe of tinfoil, incloſed between two plates of wax: in the inſtant of the paſſage the wax becomes as it were tranſparent, and is really ſeen internally to lighten.

354. This experiment appeared to me intereſting and worthy of being repeated: in order therefore to aſcertain the fact, that the light did not barely ſhine through the wax, but really extended itſelf through its ſubſtance, I inſerted two ſmall braſs rods into a thick wax candle, ſo as to make them meet in the middle; then ſending the ſpark through them, I ſaw the candle filled, in that part which ſurrounded the place of the contact, with a light that grew gradually leſs in proportion as the diſtance from that place became greater.

355. In order to enquire whether ſuch quality was peculiar to inſulating bodies, I afterwards repeated the ſame experiment with candles of tallow, of pitch, of cement, of ſulphur, of colophony, of ſealing-wax, and laſtly with one that was made with a mixture

of colophony, ſulphur, and roſin. I alſo tried the experiment by applying the two points to the ſurface of a piece of porcelain, and faſtened them upon it with a thick ſtratum of wax; I experimented in the ſame manner with a piece of opaque white glaſs; and always in its paſſage the ſpark diffuſed a moſt ample vivid light throughout the above mentioned ſubſtances, which however exhibited different colours within them: and thoſe ſubſtances, which were of a ſomewhat brittle nature, ſuch as cement, ſulphur, &c. had been previouſly warmed a little, leſt they ſhould break.

356. Who could therefore help thinking, that thoſe lights we mention, proceed from a vibration of the electric fire particularly inherent in ſuch bodies? I indeed confeſs, that I never was able to ſee any light ſhine within the ſubſtance of a china cup, in the act of charging it: the ſparks that ran from the outſide of it into my hand did not enable me to perceive the black iron-filings that I had put into it; but this does not however deſtroy the hypotheſis. It might very well be that the reaction of the ſparks ruſhing out of the outſide of the cup, being equal to the action of thoſe that were accumulating on the inſide, did not allow to the fire inherent within the ſubſtance itſelf, a ſufficient vibration; no viſible lightning therefore could take place. At the ſame time, the ſhort vibrations of that ſame fire tranſmitted to the external fire the motion introduced into the internal one, in the ſame manner as in a ſeries of elaſtic balls, a ſhock from the ball A will drive only the laſt one F, and leave all the intermediate ones B, C, D, E, in their reſpective places.

357. But ſuch a compariſon drawn from the mechanic ſhock of balls, would not entirely account for all the phenomena of the charges and diſcharges of glaſſes. It would, at moſt, explain how the exceſſive fire can accumulate itſelf on the one ſurface, when it becomes enabled to drive the natural fire from the other; and how it happens that this ſame fire, as it were, runs back, and never accumulates itſelf when the oppoſite ſurface comes to be inſulated; which is owing to the ſeries of parts that are to be vibrated, being then, as it were, infinite. The ſame compariſon would perhaps alſo explain how the fire drawn from the one ſurface, cannot be reſtored to it, but inaſmuch as the exceſſive fire accumulated on the

other furface can be driven from it: but it would not explain the two following very material circumftances, viz. how it comes to pafs, that the natural fire cannot be drawn, but as far as a quantity of exceffive fire can run to the oppofite furface; and how the exceffive fire accumulated on the one furface, cannot be diffipated, but when the natural one can return to the other.

358. In order to fatisfy the reader with refpect to all the above mentioned circumftances, we might perhaps have recourfe to a certain manner in the pofition, or adhefion, of the particles of the electric fire in the glafs, either with each other, or with the glafs itfelf. But where is the fyftem in nature, about which we are not to entertain doubts, if we undertake to meafure its probability from the extent of our comprehenfion? The connection between the two oppofite electricities is evident from all the experiments and obfervations that have hitherto been made; our inability completely to evolve and explain fuch connection, fhould perfuade us of the fhortnefs of our views, but by no means induce us to deny what we fee, or hurry us into fictions which at firft pretty well agree with the facts; but afterwards, when attentively confidered, are found to be totally repugnant to them. This precipitation we mention, feems to be the cafe with certain perfons, who, too fen-fible of the difficulty of explaining the caufe of the two contrary electricities, raife a fort of rumour againft the Franklinian Theory, and choofe to explain the above facts, by the exiftence of two diftinct fluids; but, for all their endeavours, they cannot make us believe that two fluids run from each other feveral miles, in confequence of their tendency to be more clofely united together.

359. But perhaps I have dwelt on this fubject more than was neceffary: let us return to facts. I fhall conclude this article with relating the experiment with which in the N. 168 of *Terr. Atmof. Elec.* I proved the equality of the actions exerted on the two furfaces of a plate of glafs by the exceffive fire when it runs from the one, and the natural fire when it returns to the other: we fhall derive thence more information than from any hypothetical dif-quifition in which we might engage. This is the experiment: I faften with fealing-wax the extremities of the two filk ftrings A, P, (Pl. VI. fig. 11.) which are fix feet long, to a coated glafs

plate, A B, and I ſuſpend the ſame by theſe two ſtrings: ſo that it freely hangs in a vertical plan. II. Near the centre of oſcillation, I annex to both coatings two half-balls of braſs, C D, which exactly correſpond to each other. III. Two inſulated men, the one communicating with the Machine, alſo inſulated, and the other with the Chain, touch the oppoſed ſurfaces of the plate, each with a ſtripe of tinfoil, for fear of agitating it, and it thus takes its charge. IV. This done, I place myſelf in ſuch a ſituation that I may ſee at the ſame time the two half-balls, and preſent to both at once the two heads of a conducting bow *; then the diſcharge is immediately completed, and not the leaſt agitation perceived in the plate.

360. I make the ſame experiment with ſtill more exactneſs, though with an apparatus ſomewhat more complicated. (Pl. VI. fig. 11.) I inſulate, on two little columns of glaſs, a braſs rule, L N, from which hang two metallic pendulums, E F, G H, terminated into two balls of braſs. I then place the braſs rule over the plate, ſo that the two pendulums hang on each ſide of it, and almoſt touch the half-balls C D; they are only prevented from actually touching them by two threads, F I, H K, by which they are tied from behind to the two little columns of glaſs. Things being thus diſpoſed, the charge is begun, and in the mean while the two pendulums are raiſed and kept cloſe to the braſs rule; when the charge is completed, I let fall both pendulums at once, and they at the ſame inſtant reach the two half-balls, at leaſt as near as they are allowed by the two threads that are to ſtop them; thus the diſ- is effected, and the plate remains unmoved.

361. Fourth Queſtion. *Whence the fracture of the glaſſes obſerved by Dr. Prieſtley?*

Dr. Prieſtley made an experiment with a battery of one and forty glaſs jars, having each a coating a foot ſquare, and often in the act of the diſcharge, one, two, or more jars happened to break;

* Inſtead of my uſual conducting bow, I uſe, on this occaſion, a braſs wire, or ſtring of a harpſichord, that I may eaſily bring the two knobs together; and inſtead of the uſual knobs, or braſs balls, which terminate my conducting bow, I uſe, on account of their lightneſs, two of thoſe ſo very light and thin golden balls which our country women call *dorini*, and uſe to wear in their necklaces, or trinkets.

ſometimes

ſometimes in ſeveral places. In a battery of thirty-ſix jars, having a coating a ſquare foot each, I often happened to find four of them broke, always one at a time; but for three different times I have only perceived the breaking of the glaſs ſeveral days after uſing the battery; whence I may doubt whether ſuch breakings were not merely accidental, like thoſe of glaſſes not ſufficiently heated again in the furnace, and in ſome places too thin. I have ſeen only once the corner of a jar lighten while I charged it for the ſecond time in the ſame hour, and found it broke; but I could diſcover nothing more than what had been ſeen by Dr. Prieſtley. Moreover, one day as I was watching for the moment when the bottle, A B (Pl. II. fig. 5.) ſhould ceaſe to give ſparks, I ſaw a ſpark thrown from E, where the handle was faſtened to the body of the bottle, and the disjointed handle remained in my hand: I was quick in catching the bottle with my other hand, and only perceived that the handle had been imperfectly faſtened, and could draw no kind of information from all theſe fractures in my glaſſes; I only remarked, that after ſtopping with ſealing-wax the hole made in the bottle, it could, as it did before, receive any degree of charge.

363. But though I have been unable to analyſe by experiments theſe accidents in the glaſs, yet I ſhall hazard ſome conjectures with reſpect to them. I. I ſuſpect that the diſcharge of the jars that compoſed the battery of Dr. Prieſtley was not really diſcharged at once; ſince we have ſeen that a diſcharge, when it has a great ſpace to run through, meets with ſome degree of reſiſtance, and grows divided. II. Therefore I ſuſpect that the exceſſive fire introduced into thoſe jars which are nearer the places through which the communication is formed, ruſhing at firſt very compact and denſe into the knob of the conducting bow, is repercuſſed backwards, and encreaſes in ſome degree the denſity of the exceſſive fire that is retarded in ſome other jar: to ſuch ſudden repercuſſion I think we muſt impute the different fractures of the glaſſes.

364. It ſeems to me that this hypotheſis would account for the various accidents obſerved by Dr. Prieſtley with regard to the breaking of glaſſes. I. It would explain their multiplicity: it is very natural

natural to think that a ſtroke from the fire contained in thirty-five jars, againſt that contained in ſix (this is the greateſt number that has broke with Dr. Prieſtley) may exert itſelf at once on all their weakeſt parts. II. It would alſo explain why ſuch fractures obtained, whether he effected the diſcharge himſelf, or whether it ſpontaneouſly took place. III. The ſame hypotheſis would alſo explain the moſt ſurpriſing accident of all, which is, that though ſome jars were broken, the effects of the diſcharges on the body through which the communications were eſtabliſhed, ſuch as melting and diſſipating iron wires, were equal to thoſe obſerved when all the jars remained whole. The reaſon of this is, I think, that in the diſcharge of ſuch a battery the *momentum* of the diſcharge is conſtituted by that portion of fire which, immediately and at firſt, ruſhes and forms the ſpark; and the exceſſive fire repercuſſed from a few jars, and kept back, is only that which ſerves, not to render the body of the ſpark more effectual, but to form a kind of *trailing* after it, which trailing is ſo conſpicuous in lightning. But to repeat here what I ſaid before, I here only mean to offer my conjectures on this ſubject, which I propoſe to continue to inveſtigate by obſervations and experiments; and I ſhall always be ready to embrace any opinion, well ſupported by facts, that may be propoſed to me.

CHAP. VIII.

Of the Charges and Diſcharges produced by Friction.

365. THE excitation of the electricity by the means of a friction is an object, till now, as dark as it is important. I think I have, in the appendix to my firſt chapter, explained clearly enough the reaſon why, of two bodies that reciprocally perform a friction on each other, the one, rather than the other, imparts its fire. Now I propoſe to enquire what accidents take place in an inſulating body that *receives* the electric fire, for inſtance, from the hand; whence we may alſo conclude what muſt be the caſe with an inſulating body, whoſe nature is to *give* it, for inſtance, the ſealing-wax, which gives its fire to the hand.

366. And

366. And firft of all, it appears from experience, that *it is not neceffary, in order to render an infulating body pofitively electrified by friction, that a contrary electricity fhould arife in the oppofed furface to that where the former is produced.* In fact, an electricity by excefs is very well excited by friction on a folid thick piece of glafs; an electricity by deficiency, on an oiled ftick, or a ftick of fealing-wax. Now, the manner in which I excite thofe electricities, viz. by rubbing only the edges of the plate A B, (Pl. I. fig. 10.) fhews that the rife of a contrary electricity on the furface oppofed to that which is rubbed, is fo little neceffary, that an electricity of the fame nature may very well be excited on both furfaces of an infulating body, at the fame time, if they be rubbed jointly.

367. It refults alfo from experiments, that *when only one furface of a piece of glafs is rubbed, no electricity ever takes place on the oppofed furface, but after the rubbed part has moved from the hand or body that performs the friction. Then, and then only, the exceffive fire depofited by the hand in the act of the friction, difplays itfelf on that part which has juft been rubbed; the one part of which fire runs back into the next extremity of the rubbing body, and then forms what we called above, the departing light; the other part ftops and accumulates itfelf on the glafs, and then vibrates, and drives the natural fire inherent in the oppofite furface.* The following is, among many others, the fimpleft experiment that demonftrates the above propofition. Let the circle *a b c d* (Pl. VI. fig. 12.) exprefs the plate of which I commonly make ufe inftead of a globe or cylinder; and S, one of the rubbing cufhions, the other being taken off in this experiment. I prefent a flax thread exactly to that part of the glafs, which, with its oppofite furface, preffes upon the rubbing cufhion; meanwhile another perfon makes the plate turn forwards in the direction *a b c d*, and no motion takes place in the thread near the place in contact with the cufhion, but the fame is obliquely attracted towards *a*, that is towards that part of the glafs which has juft left the rubbing body: I repeat the experiment feveral times, and the refult proves conftantly the fame.

We muft therefore conclude this, viz. *that the exceffive fire depofited on the glafs by the rubbing body, is not able, in that very inftant when it is depofited, to vibrate the natural fire from the oppofite furface:*

the

the friction only feems to increafe the natural electric capacity of the glafs in that part which is actually rubbed; fo that the fire added to it through the friction, becomes as it were natural to it, and nowife redundant; and therefore makes no effort againft the natural fire of the oppofite furface.

368. *Hence it happens that the friction introduces no beginning of a charge, except where the rubbed part begins to efcape from the contact. In that part alone the rubbed furface refumes its natural electric capacity, fo that an accumulated fire then begins to difplay itfelf, and endeavours to introduce a deficiency on the oppofite furface, by vibrating and driving the natural fire from it.* The truth of thefe principles may be rendered confpicuous by experimenting in a dark place, and prefenting a point to the above mentioned plate, (Pl. VI. fig. 12.) and making the fame turn with a fufficient degree of rapidity. So long as the point is directed to the rubbing-cufhion, no light appears on the plate. In order to have the little ftar fhine on the point, the latter muft be prefented a little beyond the cufhion: nor muft it be prefented precifely to that part where, at the parting of the plate from the hand, the *departing* light appears, but a little farther: for it feems that the fire requires fome fmall fpace of time to be able to manifeft itfelf, and drive the natural fire from the oppofite furface.

369. We have therefore analytically difcovered here a new and wonderful way of charging glafs: the fame man may, with one of his hands, give extenfive fire to the one furface of the plate, and, with the other hand, take the natural fire from the other furface of it: the fame man can perform the two functions of both an animated fyftem, and an indifferent fyftem!—I take from its place the rubbing-cufhion S, (Pl. VI. fig. 12.) I rub the plate with the palm of my hand, and with my other hand I prefent a very fharp point to the oppofed fide of the plate, in *a*, a little beyond the *parting* light, and I order the fame to be turned evenly. At firft a very vivid fpark fhines on the point; then it grows faint by degrees; the *parting* light, on the contrary, grows continually more copious, and at laft fhews that the charge is completed. I then remove, at the fame inftant, both my hand and the point, and order the motion of the plate to be ftopped: then I take a brafs rod, one line thick,

thick, rounded at its end; and, at the same time that I successively present my hand to the different parts of the rubbed surface, I present the brass rod quite close to those parts of the other surface which correspond to them, when vivid brushes spring from every part with a rattling noise; and thus I complete the discharge by the means of a number of successive brushes. I may also silently effect the same discharge, by applying both my hands to the two opposite surfaces: I can also effect it with a stroke, viz. in coating the two surfaces, one at a time, and then touching them both at once.

370. And this method of producing a charge by friction, however surprising it may appear, as it is effected by a single person, or to express it in more universal terms, by the same conductor, yet is so clear, that it scarcely wants an explanation. I. The friction from the hand increases the natural electric capacity of the glass; therefore, a quantity of fire flows from the hand into the glass. II. That part of the glass which escapes from the friction, resumes its lesser capacity; the deposited fire then manifests itself on it, and drives the natural fire from the opposite surface into the brass rod which is presented to it. III. When, afterwards, the glass by turning gets again under the rubbing hand, the friction increases a-new the natural capacity of the rubbed surface, as far as the quantity of natural fire which the opposite surface has before lost will allow it, therefore, deposits some additional fire upon it; till at last an equilibrium is produced between the force of the friction for increasing the electric capacity of the rubbed surface, and the resistance from the other surface against sending any more of its own fire.

371. The discharge is likewise very easily understood; in order to effect it, it is enough to present the point to the inside of the tube, opposite to that part which is rubbed. I said before, that when I present the thread, or the brass rod, opposite to my hand, or any other body that performs the friction, the rod draws no fires and the thread is only obliquely attracted towards that part of the glass which has just escaped from the friction. But this, we must observe, only obtains in the beginning of the operation; for if the charge has been farther promoted after the manner we have just

 described;

defcribed; then, the thread is directly attracted; and then, if we prefent the point oppofite to the rubbed place, a very vivid brufh rufhes from the point, which deftroys the charge.

372. This experiment confirms two truths, and points out a third. I. It confirms this truth, *that the exceffive fire depofited by the hand on the rubbed furface, diffufes itfelf into that furface without making the leaft effort (during the immediate act of the friction) to drive the fire from the oppofite furface; but diffufes itfelf into the glafs in confequence of, and as due to, the then greater capacity of it.* II. It confirms this fecond truth, viz. *that it is only beyond that part where the friction is performed, that the depofited fire manifefts itfelf; that is, at that place at which the glafs returns to its leffer natural capacity; and it is likewife at this fame place, that the natural fire begins to be driven from the oppofite furface.* III. The fame univerfally demonftrates, *that that part of the furface of the glafs which is oppofed to that part of the other furface which is rubbed, is exactly in the fame difpofition as to receiving back the natural fire of which it has juft been deprived, or an additional fire, or again to throw its own fire, as if the other furface were not rubbed.*

373. Thefe truths prepare the way for producing and analyfing another method of forming a charge by friction; I mean to fpeak of that kind of charge which is effected by rubbing the naked furface of a glafs the other of which furface is coated. The experiment has been firft invented by Dr. Franklin, who, by his vaft and penetrating genius, has, without any farther affiftance, found the hypothefis of the *natural electric capacity* of the glafs being increafed by the friction. Place, fays he, at p. 80, a wire into the tube, " the inward end in contact with the non-electric lining, fo it " will reprefent the Leyden bottle. Let a fecond perfon touch " the wire while you rub, and the fire driven out of the inward " furface when you give the ftroke, will pafs through him into " the common mafs, and return through him when the inner fur- " face refumes its quantity, and therefore this new kind of Ley- " den bottle cannot be fo charged. But thus it may; after every " ftroke, before you pafs your hand up to make a another, let the " fecond perfon apply his finger to the wire, take the fpark, and " then withdraw his finger, and fo on, till he has drawn a

" number

" number of ſparks; thus will the inner ſurface be exhauſted, and " the outer ſurface charged. Then wrap a ſheet of gilt paper cloſe " round the outer ſurface, and graſping it in your hand, you may " receive a ſhock, by applying the finger of the other hand to the " wire; for now the vacant pores in the inner ſurface reſume " their quantity, and the overcharged pores in the outer ſurface " diſcharge their overplus."

374. This explanation does not, however, comprehend all the admirable accidents I obſerve in ſuch charge. I line one half part of the inſide of the tube B A (Pl. IV. fig. 3.) three feet long, and one inch and an half in diameter; I make a few inches of the lining hang out of the tube in B C, that I may annex two threads to it; I obſerve to graſp the tube by its unlined part D A; and rub it from B towards D; keeping my hand at a diſtance from the tube, when I bring it back to B, in order to begin again the friction.

375. And I obſerve, I. That in the act of friction, eſpecially at the firſt, no electricity is manifeſted either from the outer, or the inner ſurface of the tube; a thread hanging cloſe to the outer ſurface, does not move in the leaſt, nor do the threads in C diverge; but when I ſuddenly ceaſe to rub, and remove my hand from D, then the threads in C diverge, and that annexed to the outer ſurface of the tube is attracted.

376. II. I obſerve, that the charge is ſo formed, that it is greateſt near B, where I begin the friction, and from thence towards D, it gradually leſſens. In order to aſcertain this, I do not coat the outſide of the tube; for in this caſe I would only obtain one general diſcharge, from which I could perceive no difference between the ſeveral charges of which it is made up; but I ſucceſſively graſp different parts of the naked tube, when I obtain ſeveral diſcharges, which become gradually leſs, as they are ſucceſſively drawn from a part nearer to D.

377. Now, in order to explain theſe accidents with a ſufficient degree of clearneſs, I conſider the outer ſurface of the tube B D, as if it were divided into ſmall and equal circles, A, B, C, D, E, F, ... Y, Z; and I ſuppoſe the inner ſurface to be likewiſe divided into correſponding circles, *a*, *b*, *c*, *d*, *e*, *f*. . .*y*, *z*. Then I ſay, all the fire, which, in conſequence of the juſt performed friction, is vibrat-

 ed

ed from the precedent circles *a, b, c, d,* immediately runs through the lining into the neareſt circles *e, f, g, h,* which the rubbing hand at that very time graſps; ſo that when the hand, at the concluſion of the friction, becomes applied to the circles V, X, Y, Z, the whole fire that has been driven from the preceding circles, is affixed to the circles *u, x, y, z.* And thence may be underſtood how it comes to paſs, that, in the act itſelf of friction, the threads in C do not diverge, and that annexed to the outer ſurface is nowiſe attracted. At the ſame time, that the circles A, B, C, D, eſcape from the friction, the fire that has been depoſited on them, and which, if the tube were not lined, would manifeſt itſelf by viſible ſigns, is only *accumulated* and the natural fire which it has been able to drive from the inner circles *a, b, c, d,* through the lining, goes through that ſame lining to accumulate itſelf on thoſe of the following circles, that correſpond to the rubbing hand, and introduce there a *conſequent* charge; therefore, the exceſſive fire, ſince it is accumulated and forms a charge, does not attract the thread annexed to the outer ſurface of the tube; and, for the like reaſon, the natural fire accumulated on thoſe inner circles to which the hand is applied, does not attract the threads in C.

378. I ſay, in the ſecond place, that the fire depoſited by the rubbing hand on the laſt circles, decreaſes as the quantity of the fire depoſited on the former circles increaſes: the reaſon of this is, that the ſum of the fire which from the inner circles *a, b, c,* runs to accumulate itſelf on thoſe which are neareſt to them, is equal to the ſum of the fire accumulated on the outer circles A, B, C. Now the fire which is thus accumulated on the inner circles *d, e, f,* endeavours to introduce there a conſequent charge, endeavours to drive the natural fire from the outer ſurface, and the fire which the rubbing hand is to accumulate on that place in the act of friction, muſt needs meet with a kind of oppoſition, and does little more than preſerve the ſame place in its natural ſtate; therefore, in proportion as a greater quantity of fire will have flowed from the firſt circles *a, b, c,* to the ſucceeding circles, *d, e, f,* the quantity of fire which the hand will be able to leave on the outer circles D, E, F, will be leſs. Whence it neceſſarily follows, that the charge in the ſucceeding circles will grow gradually leſs, and will follow in that reſpect a certain proportion.

379. Let

379. Let us ſuppoſe, that the hand being firſt applied to the four circles A, B, C, D, leaves the circle A, which has already been rubbed, and now reaches the circle E; let, moreover, the quantity of fire which the rubbing hand has depoſited in A, be called 1; from the inner circle *a*, the driven fire will paſs into the circles *b*, *c*, *d*, *e*; and leſſen, by a fourth part, both the fire which the rubbing hand is to drive from them, and that which it is to accumulate on the outer circles B, C, D, E.

Let now the hand leave B, and reach F; there will be in *c*, *d*, *e*, *f*, both the former fire proceeding from *a*, and the three-fourth parts proceeding from B; that is, ſeven-fourth parts in all, which, diſtributed into the ſaid circles, will leſſen for $\frac{7}{16}$ the fire which the hand is to drive from *c*, and to depoſit on C; ſo that it will only depoſit $\frac{9}{16}$.

Let the hand leave C, and occupy A; there will be accumulated on the circles *d*, *e*, *f*, *g*, both the fire from *a*, the three-fourths from *b*, and the $\frac{9}{16}$ from *c*, which will make up $\frac{37}{16}$, and this quantity being diſtributed into thoſe four circles, will diminiſh for $\frac{37}{64}$, the fire which the hand would depoſit in D; therefore, it will depoſit only $\frac{27}{64}$; ſo that writing under the circles.

	A,	B,	C,	D,	E,	F,	G,	&c.
the ſeries	1,	$\frac{3}{4}$,	$\frac{9}{16}$,	$\frac{27}{64}$,	$\frac{87}{256}$,	$\frac{243}{1024}$,	$\frac{729}{4096}$,	&c.

we ſhall ſee diſpoſed in a regular order, though ſomewhat inexactly indicated, the proportion which the decreaſe of the charge follows in the different ſucceſſive circles; whence we ſhall perceive, that the quantity of fire introduced into the latter circles never reaches to an equality with the quantity of fire which the hand would depoſit on them through the effect of the friction.

380. However, the above ſeries only expreſſes the quantity of fire depoſited by the firſt friction; the quantities that the hand will depoſit by ſucceeding frictions on the laſt circles, will gradually increaſe in proportion as the ſum of the fire already driven from the firſt ones, and extracted by the means of the ſparks drawn from C (Pl. IV. fig. 3.) will be greater; and as the fire which, in conſequence of the friction of the firſt circles A, B, C, D, will be driven from the inſide ones, *a*, *b*, *c*, *d*, into the ſucceſſive ones *l*, *m*, *n*, *o*, will thus grow continually leſs, it is very poſſible, that by repeating the frictions a great number of times, and continually

continually drawing off the ſparks, at length a complete charge, and equal throughout the tube, may be obtained.

381. But without loſing time in inquiring after what is poſſible, let us attend to that which is practicable; let us firſt explain a few accidents attending that method of producing a charge, which accompany thoſe above mentioned, and at firſt ſight appear very ſtrange. After rubbing the tube ſeveral times, and drawing off the ſparks from C (Pl. IV. fig. 6.) when I again apply my hand near B, in order to rub one ſtroke more, it happens, I. That the threads in C, which were fallen in conſequence of the drawing a ſpark from C, diverge again, being now electric by deficiency, and the tube appears likewiſe as if it were electric by deficiency at its extremity; all which, however, is quite conformable to the theory. When I apply my hand to B, I leſſen the exceſs that obtains there; therefore, I leſſen the deficiency in the correſponding inner circles, to which a certain quantity of fire can flow through the metallic lining, either from other inner circles, or from that part of the lining which hangs out in C (we ſhall in the next chapter ſee that no fire can be extracted from the inward ſubſtance of the lining, or be accumulated within the ſame); hence it becomes neceſſary, that the threads in C ſhould diverge in conſequence of the deficiency introduced into them; and what little fire flows from the inner circles correſponding to D, to the inner circles near B, is alſo ſufficient to make the outer parts of the tube near D appear electric by deficiency.

382. I do not mention this circumſtance, that when I do not apply my hand to B, till after rubbing a number of ſtrokes and extracting the ſparks from C, the threads in C diverge ſtill more than before; becauſe it is obvious, that as the introduced exceſs is then greater, it of courſe leſſens, when the tube is touched by the hand, in a greater proportion than in the former caſe, therefore, the deficiency in the inner correſponding circles likewiſe leſſens more, and a greater quantity of fire runs to them, both from the other inner circles, and from the portion of the lining which hangs out in C.

383. I omit likewiſe, that when I continue rubbing, after a great number of frictions are already performed, and a number of ſparks have been drawn, the thread annexed to the outer ſurface is ſomewhat

what attracted by the tube, even during the act of friction, and the threads in C lofe their divergence by deficiency, and begin to diverge by excefs; fo that the greateft attraction of the former, and divergence of the latter, always take place at the inftant when I remove my hand. The reafon is, that as the introduced charge is become greater, a portion of the fire depofited on the preceding circles, begins to exert itfelf, though during the act of friction, (in the fame manner as we fee that a greater portion of fire is fent into the Chain, and manifefts itfelf vifibly around it, according as the excefs in the cylinder of glafs is greater); and this fire which manifefts itfelf on the outer furface, will caufe a fmall portion of that which is driven by the action of the inner circles, in C, to manifeft itfelf.

384. I fhall now pafs to the explanation of other accidents which are common both to the charge of the above-mentioned tube, and to a few other celebrated experiments. It is well known, that if a cylinder, fuch as that in Pl. III. fig. 14. be lined, either intirely, fo that the lining communicate with the ground, or only in that part of it F, O, E, N, which is rubbed, it will communicate no electricity to the Chain. I annex to the Chain a bunch of metallic threads, or wires, that hang over the cylinder of this fig. 14; they fpread themfelves over that part which is inwardly lined, and, befides extend themfelves over the unlined part towards N. If, by ftretching one of my fingers, I rub the unlined part in N, the Chain becomes electrified; if I rub only the zone inwardly lined, no electricity arifes in the Chain.

385. This cafe is like that of the tube which caufes no divergence in the thread in C (Pl. IV. fig. 3.) and does not draw the outfide thread at the firft friction, and never can be charged, unlefs after every friction a fpark is drawn from C; fuch tube, therefore, exactly reprefents the above cylinder with an infulated lining ON (Pl. III. fig. 14.) and this cylinder may be charged equally well as the tube, if it be managed after the fame manner. Thus, I remove the Chain from the cylinder, then alternately rub it, and alternately infert a rod through a hole I have bored through one of its wooden armatures, and draw a fpark from the lining ON. After drawing thus a number of fparks, I take the cylinder from the fcrews, and place the rod *u n* (Pl. III. fig. 7.) through the hole;

the mean while another perſcn holds the cylinder by a ſilk thread; I outwardly coat it in E F; and as ſoon as I touch the rod in *u*, with the head C of the conducting bow C T *e*, there ſprings in *n* a moſt conſpicuous *bruſh*, and a *little ſtar* ſhines in *c*.

386. In the ſame manner, if the lining of the ſame tube communicates with the ground, it will exactly repreſent the cylinder communicating with the ground, and contract no electricity.

387. Now, in all theſe experiments, either with the tube, or the cylinder, there is this common to all. I. That the fire depoſited by the rubbing hand on the rubbed ſurface, does not manifeſt itſelf by outward ſigns when this ſurface eſcapes from the hand, (as is the caſe with unlined glaſſes); but accumulates itſelf within the glaſs, and there produces a beginning of a charge; the reaſon is becauſe the natural fire may then from the inward ſurface flow away through the lining. II. In all the above experiments, that ſmall quantity of natural fire which is driven from the inward ſurface by the ſmall portion of exceſſive fire which accumulates on the outſide, is always to be found in the lining.

388. The effect produced by the firſt of theſe two circumſtances is, that the firſt friction ſends no perceivable electricity to the Chain, and from the ſecond circumſtance it reſults, that the following frictions ſend no more than the firſt. The firſt friction ſends no electricity to the Chain, becauſe, as the ſmall quantity of fire depoſited at firſt by the rubbing hand, drives an equal quantity from the inward ſurface, it only introduces a beginning of a charge. To underſtand this better, graſp a bottle hanging from the Chain, and give a ſpark to the latter; the electrometer annexed to it will not for all that move, at leaſt viſibly; this is becauſe there is the ſame proportion between that portion of the ſpark, which, in ſuch caſe, remains around the Chain and can manifeſt itſelf, and that which goes to the bottle, as there is between the electric capacity of the Chain and that of the glaſs-bottle, which is incomparably larger: now, the ſame will take place in the caſe of the cylinder we ſpeak of, when, after the firſt friction, it will be in contact with the Chain. Nor will ſubſequent frictions augment ſuch charge; becauſe the ſmall quantity of fire accumulated by the firſt friction will get back into the hand, when the ſame part of the glaſs returns to it; and it will be

be able to do, ſince the natural fire can, through the lining, return back to the inward ſurface, and deſtroy the weak deficiency at firſt introduced into it; ſo that the outer ſurface will return to a ſecond friction, in the ſame ſtate as it was when it underwent the firſt; it, therefore, always moves from the rubbing body with but a ſmall portion of fire outwardly accumulated on it.

389. Beſides, if the interior lining be inſulated, the ſmall quantity of fire that iſſues from the bands, or zones, *a*, *b*, *c*, *d*, &c. (I conſider the outer ſurface of the cylinder as divided into a ſeries of bands, or zones, A, B, C, D, and the inward one into another, *a*, *b*, *c*, *d*), endeavours correſpondently to the bands A, B, C, D, to flowback into the ſubſequent bands *e*, *f*, *g*, *h*, to which the hand is then outwardly applied, as it happens in the tube above-mentioned; and ſuch fire, after one turn of the cylinder, finding itſelf wholly accumulated againſt the place of the friction, will repel all the fire which the hand may ſeek anew to depoſit on the ſame.

390. This fact, that the electric fire ſucceſſively runs from thoſe places which are outwardly left by the hand, into the ſubſequent inward ones to which the hand becomes outwardly applied, is viſibly demonſtrated by one of the moſt pleaſing electric experiments yet made, and reciprocally affords a conſpicuous explanation of that ſame experiment, which would otherwiſe be one of the moſt difficult to underſtand. This experiment is become too important not to be minutely deſcribed. I take the cylinder B A C (Pl. II. fig. 1.) with a braſs armature in B D F; the female ſcrew E may be fitted to the male ſcrew that riſes from the baſon of the air-pump; the ſmall key D ſerves to preſerve the vacuum, the ſcrew F is fixed into the female E, and is made a little hollow at its extremity F, ſo that it may be fixed to one of the ſcrews V, T (Pl. I. fig. 2.)

391. Having carefully emptied ſuch cylinder of air, and placed it in its proper place, I apply my hand to it and rub it; which is ſcarcely done, than I have the agreeable ſight of my hand being repreſented on the inward ſurface of the ſame, by a red violaceous light, ſurrounded by another of a more vivid and whiter colour. The image of my hand thus repreſented, is delineated in Pl. IV. fig. 2; the fleſhy prominent parts of it which touch the glaſs, are

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reprefented by a violaceous light, languid, rare, and uniformly fpread with obfcure points; the hollow intervals between the prominent parts that perform the friction, appear filled with a ftronger and whiter light, which laterally extends itfelf along thefe fame parts.

392. I have repeated the experiment after the fame method ufed by Mr. Haukfbee, its inventor, with a cylinder inwardly lined with fealing-wax, with another lined with fulphur, and another lined with pitch; and in all thefe, when emptied of air, and rubbed in the fame manner, I enjoyed the fame fight as mentioned before; in all, the hand was reprefented on the thin inward lining: though, in order to fee this, it was neceffary to look through one part of the glafs which had been left unlined: through the lining of fulphur, of wax, or of pitch, no light at all could be perceived.

393. I have juft now explained how the fire depofited on the bands A, B, C, D, the act of friction, drives the natural fire from the inwards bands *a, b, c, d*; and how this fire fucceffively runs through all the other fubfequent bands; now, this fame explanation will alfo ferve to account for the wonders mentioned by Mr. Haukfbee with refpect to the reprefentation of an object within an opaque body, without the help of any looking-glafs, or of any lamp or candle; and about the electric tranfparence of fealing-wax, of fulphur, and pitch; bodies through which no light from a candle can be perceived.

394. In fact, it is not light which traverfes the fubftance of the cylinder, that produces the above reprefentations, as Mr. Haukfbee thought it was; nor is it electric fire only vibrated in the place itfelf of friction (as I gave my opinion in the book of *Terr. Atmof. Elect.*) it is electrical fire that is vibrated and driven from the inward bands *a, b, c, d,* where the outward ones A, B, C, D, firft efcape from the hand, and which runs through the fubfequent bands of the glafs, or of the linings (thofe linings being electric *per fe*, and able to be charged like the glafs, make one body with it) which runs, I fay, after a moft confequent manner, to form the above reprefentation.

395. Affoon as the rubbed glafs leaves the hand, the electric fire depofited on it then accumulates, and affixes itfelf

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on it, and drives the natural fire from the inner furface: this fire thus driven, follows, on that inner furface of the glafs, or of the linings, thofe parts to which the hand becomes by turns outwardly applied, and lightens exactly in the fame manner as the electric fire does when flowing on the inner furface of a glafs jar emptied of air; and if we apply to every rubbing part of the hand, what is here faid of a few, we fhall underftand how the whole lucid image before mentioned arifes, with all its peculiar circumftances.

396. Correfponding to that part of the finger which outwardly touches the glafs and rubs it, the light will appear of a red violaceous colour, (we fhall fee afterwards how the rarity of the electric fire produces fuch colour), and befides be languid, rare, and uniformly fpread with fmall obfcure points; this is becaufe both the fmall cavities in the fingers and the prominences on them, are in that place very clofe to each other; therefore there is fcarcely time fufficient for the fire correfponding to fuch cavities to be vibrated and to lighten, as it prefently finds itfelf carried over-againft the rubbing prominences, correfpondently to which it affixes itfelf on the inner furface.

397. But correfpondently to thofe parts of the fingers which lie near their fides, and where the cavities we mention turn up round them, and grow wider, as there is both more room and time for the fire left by the prominences of the finger on the outward glafs, to affix itfelf on it, and confequently for the inward fire to be driven from the inward furface, there this fire will flow more compact and copious on the faid inward furface, and therefore will exhibit a whiter and fteadier light.

398. But, after all, why does the fire vibrated from the bands, *a*, *b*, *c*, *d*, inftead of fpreading itfelf every where, continually follow, and keep to the fubfequent bands *e*, *f*, *g*, *h*, &c. to which the hand becomes fucceffively applied?

399. The reafon is, that the fire, while it flows along thefe fucceffive bands, meets with the leaft poffible refiftance. In fact, though the cylinder is ever fo carefully emptied of air, there is always left within it fome fmall remnant of it, which (fuppofing the common height of mercury in the barometer to be twenty-feven inches and an half, Italian meafure,) will, if the vacuum be promoted

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even ſo far as to lower the mercury to one-twelfth part of an inch, will, I ſay, be equal to one-three hundredth part of the atmoſpheric air : the reſiſtance therefore which the vibrated fire will meet, when ſeeking to ſpread itſelf in every direction through this medium will be proportioned to its denſity; and this reſiſtance will encreaſe, according as the fire will ſeek to ſpread itſelf to a greater diſtance; wherefore it muſt keep along the above-mentioned ſeries of the bands *e*, *f*, *g*, *h*, &c. which continually afford it the neareſt paſſage. With regard to flowing backwards along thoſe bands which are juſt rubbed, the fire will be prevented from doing it by the ſame force by which it is driven from the glaſs; that is, the ſame exceſſive fire that affixes itſelf to the bands A, B, C, D, and drives the natural fire from the bands *a*, *b*, *c*, *d*, will of courſe hinder it from returning to them; therefore, the fire driven from the bands *a*, *b*, *c*, *d*, will meet with a greater reſiſtance while it attempts to flow, either ſidewiſe through the above-mentioned remnant of air, however rare it may be, or backward along the inward zone, than in immediately flowing from the precedent band *c*, to the following and contiguous one *d*, where no fire is yet affixed on the correſponding outward band D, ſince that band being, as we ſuppoſe, actually rubbed, has only a quantity of fire proportioned to its then encreaſed capacity.

400. The ſame reaſon holds true with reſpect to the lined tube before-mentioned, as well as with reſpect to the cylinder we are now deſcribing. If the metallic linings of either this tube or cylinder only tranſmitted the electric fire along their outward and open ſurface, as is the caſe with glaſs, or inſulating linings made of wax, ſulphur, &c. no doubt but the ſame images would be perceived on thoſe metallic linings as are to be ſeen on glaſs, or on the inſulating linings we mention. And, vice verſa, if either glaſs, or thoſe linings, had the property of tranſmitting the electric fire to ſome diſtance forward, through their own inward ſubſtance, as metallic linings do, no image would then be ſeen upon them, any more than on metallic linings.

401. But let us proceed to deſcribe the other accidents attending this experiment, which are not only both curious and uſeful to confirm the explanations already given, but alſo afford an explanation for

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other phenomena, which yet ſeem at firſt to have no connexion with that ſame experiment.

Having ſufficiently conſidered the lucid image or reflection of my hand, or other bodies apt to perform a friction, I open, for an inſtant, the little key D, (Pl. II. fig. 1.) and give entrance to a ſmall quantity of air: which done, I obſerve, I. That the electroſcope of the Chain already begins to diverge a little, though it did not move at all before; and when I preſent my finger quite cloſe to the Chain, I begin to ſee a ſmall light manifeſt itſelf from it. II. The ſurface of the cylinder, where it leaves the rubbing hand, begins to attract a thread. III. The image of the fingers grows languid by degrees. IV. Inſtead of it, a light, *a*, *b*, *c*, ſhaped like a *nail*, (Pl. II. fig. 4.) begins to appear, and ſpreads itſelf over the inward bands, *a*, *b*, *c*, *d*, correſpondently to the outer ones, A, B, C, D, as they part from the rubbing hand.

402. Now, the peculiar appearance of this *nail* of light, ſhews both its cauſe, and the cauſe of all the other concomitant alterations in the image within the cylinder. It is the reſult of the fire that is driven from the interior bands *a*, *b*, *c*, *d*, by that which now begins to accumulate itſelf on the outward ones A, B, C, D. The quantity of air that has been let into the cylinder does no longer allow this fire to paſs ſo readily as it did before into the ſubſequent bands *e*, *f*, *g*, *h*: a part of it, indeed, is able to reach them, but then it only does it ſlowly in conſequence of the encreaſed reſiſtance it meets with; and, as it is kept for a while from reaching the ſaid bands, it forms, in the meanwhile, the appearance of this nail of light. As another part of the fire never can reach the ſaid bands, but remains in the ſame place on the inward ſurface, and prevents the condenſating of the fire on the outward one, the latter fire conſequently begins now to manifeſt itſelf; whence reſult both the divergence of the electroſcope, and the other ſigns in the Chain, and more immediately the attraction of the thread by the cylinder. Laſtly, it is likewiſe evident, that ſince the interior flowing of the fire correſpondently to the rubbing fingers begins to be hindered, the lucid repreſentation of the latter muſt gradually diſappear, ſince, even when the air is moſt dilated, this repreſentation is but rare, and in ſome places ſpread with dark points.

403.

403. When, for the fourth time, I let in another ſmall quantity of air, the *nail a, b, c, d,* (Pl. II. fig. 4.) already ſpreads itſelf ſo far as an inch and an half, and is terminated in a ſharp point; ſparks may now be drawn off at the diſtance of more than one line from the Chain, and the outer ſurface of the cylinder begins to attract thicker threads. A new light, beſides, is exhibited in the inſide of the cylinder; flaſhes or rays of fire now ſpring from ſuch inward points as correſpond to thoſe which outwardly get out of the friction, and croſſing the rarefied air, run to the oppoſite inward ſurface of the cylinder, with a particular incurvated motion and divergence worth being deſcribed. Let *a, b, c, d,* (Pl. II. fig. 3.) indicate the ſection of the cylinder where it is rubbed; very near the point F, where the glaſs parts from the rubbing hand, (I ſuppoſe it moves according to the direction *a, b, c, d,*) rays of light, *f, g, h,* are ſeen to ſpring and move with ſuch an incurvated motion, as make it evident that the ſame is compounded of three motions, viz. of the rectilineal motion produced by the force that drives the fire; of the rotating motion of the cylinder, of which the ray, when iſſuing from it, ſomewhat partakes; and of the diverging motion by which the rays part from each other.

404. Of theſe motions, certainly the firſt and rectilineal one originates from the condenſated outward fire, which drives thoſe rays of the inward fire; in fact, thoſe rays keep more cloſe to each other according as the motion of the cylinder, and conſequently the force ariſing from the friction encreaſes. The ſubſequent curvature of the rays is evidently the effect of the rotation of the cylinder, ſince it follows its direction, and viſibly keeps pace with the rapidity of it: whence we may conclude that the velocity of this rotation is not in any very ſmall *ratio* to the velocity of the rays themſelves through the medium in which they move; ſo that, the former being *given,* we might conclude and find out the latter. Not that I think, however, that ſuch rays are continuous, as they at firſt appear to be: the velocity with which children move lighted ſticks, and make thereby their ends appear like continued igneous ribbons, is not, we may ſafely affirm, ſo great as that with which the rays of the electric fire move; therefore, a ſingle flaſh or *darting* of this fire, though it may not of itſelf occupy a greater ſpace

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than a twelfth part of an inch, may very well produce ſuch a continued impreſſion on the eye, as to appear a continued ray throughout the long ſpace it paſſes through: which obſervation I thought neceſſary in order to hint what a ſmall quantity of fire may be ſufficient to raiſe the appearance of theſe long copious flaſhes of light.

405. It now remains that we ſhould ſay ſomething of the diverging motion of the rays which we have mentioned above; this motion, beſides what it has in common with the motions of the electric fire through the atmoſpheric air, or through air imperfectly dilated, has moreover, on this occaſion, a peculiar cauſe, viz. the electric atmoſphere of the rubbed zone, which, when the rays begin to flaſh, begins alſo to acquire a degree of intenſity; therefore this atmoſphere muſt repel theſe flaſhes or rays: (in the following chapters I propoſe to ſhew how the direction of electric bruſhes, and alſo of ſparks, is influenced by the repulſion of the electric atmoſphere of the electrified body.) ſo that they diverge from each other very faſt, and tend to move towards the lateral zones of the cylinder, which are not rubbed.

406. To a ſimilar reaſon I impute certain flaſhes of light which I ſometimes perceive in the inſide of cylinders even when filled with atmoſpheric air. Sometimes, after rubbing thoſe cylinders ſtrongly, and for a long time, I perceive on that part of them which is diſtant about two inches from the place of friction, eſpecially when I encreaſe a little the force of the latter, I perceive, I ſay, narrow flaſhes of light, which, from the inner ſurface of the glaſs, as far as I am able to perceive, run through a right line from the rubbed zone to the lateral zones that are not ſo, and there vaniſh. I think that ſuch flaſhes of light are produced by the fire vibrated by that which outwardly accumulates itſelf in very great plenty; and alſo that this fire inwardly vibrated has a propenſity to flow to the lateral parts of the cylinder, where no other fire is vibrated.

407. This conjecture I think I have aſcertained; I perforated in *n* (Pl. III. fig. 7.) the wooden armature of a cylinder; I rubbed it for a very long time; I then took it from the ſcrews; I inſerted through the hole *n* the rod *n u*; I inſulated and coated it, as I mentioned before of the cylinder that was inwardly lined with

metal; and having properly adjuſted the conducting bow C T C, I obtained the bruſh in C, and the little ſtar in *c*; ſo that even in ſuch cylinder a degree of charge took place; there was ſome exceſs on the outward rubbed zone, and a deficiency in the oppoſite ſurface: a certain quantity therefore of fire muſt needs have been diffuſed into thoſe parts of the inward ſurface which lay near thoſe from which the fire was driven; and a certain degree of dampneſs in the air would have carried ſtill farther this diffuſion of the electric fire.

408. But even this laſt circumſtance is not neceſſary: it is a very common thing in the drieſt weather to find remnants of electricity in cylinders, twelve, eighteen, or more hours, after the friction has ceaſed; which never is the caſe with plates of glaſs rubbed on both their ſurfaces at once. It ſeems that ſuch remnants of fire in cylinders proceeds from a kind of charge that takes place in them; that is, from the natural fire which, being driven from the inward zone oppoſite to that which undergoes the friction, diffuſes itſelf either into the lateral zones, or into thoſe inward parts of the cylinder which correſpond to the wooden armature; and this fire, in the cloſe air in the cylinder, returns but ſlowly to its former place. If, four hours after ceaſing the friction, you give the cylinder half a turn, without touching it, you will find the electroſcope of the Chain to diverge; which will be owing to the cylinder having diffuſed into the chain an exceſs of fire adequate to ſuch doſe of natural fire as has ſlowly returned to its *natural* poſition in its inſide.

409. But let us return to the experiment of Mr. Haukſbee. The nail of light *a b c*, (Pl. III. fig. 4.) which, when a ſmall quantity of air has been let into the cylinder, appears beyond the rubbing finger, affords a complete explanation of the light that appears in the barometer, and accompanies the mercury in it when it falls. I take the barometer and move it ſuddenly downwards, ſo as to make the mercury riſe in it at once: as the ſame returns to its level, it is followed by a ring of light exactly like that which the *nail* forms within the cylinder, and both thoſe lights vary in one and the ſame manner, according as circumſtances change: that is, if in both the vacuum be complete, the light will be extremely weak; and as a ſmall quantity of air is let in, the ſame will lengthen and ſtretch itſelf

ſelf under the ſhape of a nail; all other circumſtances between the cylinder and the barometer will alſo be found exactly alike, and the ſame explanation will conſequently ſerve for them all; the lengthened tract of light obſerved in the barometer is owing, as we ſaid before of the *nail* in the cylinder, to that portion of fire which, through the new reſiſtance from the air, can no longer readily follow the rubbing body, viz. the mercury in the barometer.

410. With regard to the attracting of ſurrounding bodies, the cylinder, we are to obſerve, begins to operate in proportion as the natural fire inwardly vibrated begins to follow leſs readily the rubbing body; becauſe the outward fire, being thereby kept from condenſating itſelf, manifeſts itſelf viſibly, and attracts ſuch light bodies as are placed near it. The ſame effects exactly take place, and for the ſame reaſon, in the barometer: as ſoon as, through the increaſed reſiſtance of the air, the exceſſive fire within it can no longer freely and readily follow the mercury in its fall, it therefore, remains behind in a ſtate of accumulation, and then begins to drive the natural fire from the outer ſurface; which fire in its turn manifeſts itſelf, and attracts threads and other light bodies.

411. Now to conclude our principal experiment. I let air into the cylinder for the fifth, ſixth, and ſeveral other ſucceſſive times, and the flaſhes of light gradually become more difficult to be produced; they now can only be obtained by ſtronger ſtrokes of friction; they are ſhorter, narrower, though more lucid, and move with much more irregularity; which circumſtances we ſhall find hereafter to be conformable to what I propoſe to ſhew in the IV. ſection with regard to the reciprocal action of air and electric fire upon each other. Here I only ſhall obſerve, that in proportion as more air is admitted into the cylinder, the electric ſigns grow more vivid in the Chain; but this only obtains within certain limits, for it appears that the ſigns attain their higheſt degree before all the air is let in; and, when it is, the ſame begin to weaken.

412. Now, this obſervation puts me in mind of the experiment of Monſ. Du Faye, who could not, by friction, draw any electricity from a tube of glaſs within which he had condenſated

air; from that experiment I was inclined to think, that the properest degree of denfity of the air within rubbed tubes for exciting a ftrong electricity, was under that of the common denfity of the atmofpherical air; but the following experiment, which I have feveral times repeated, and always with the fame fuccefs, has made me alter my opinion.

413. I made ufe of the long incurvated tube A, B, C, D, E, F, open in E, and clofed in F (Pl. VI. fig. 1.) which ferves me to fhew, that our air, when unmixed and pure, *contracts itfelf into a fpace inverfely proportioned to the compreffing weights*; I loofened the fhorter branch of it, F D, from the board to which it was faftened, and moved it from it, that I might more eafily rub it; and the longer branch remained faftened to the fame board. I could not, without danger of breaking it, rub this branch F C with any great degree of ftrength; however, by rubbing it with the amalgamated cloth, I could electrify it pretty well; I enabled it to draw threads at a foot diftance, and it could give a few fparks. Having afcertained as well as I could the quantity of this electricity, I poured mercury into the longeft branch C A, in a fufficient quantity to make it rife in the other branch C F ninety lines long, to *e*, that is to fay, to the height of forty-five lines; by that means, the air in the tube was brought to a denfity double to that in the atmofphere. I then rubbed the part F *e*, and found that the electric figns from it, inftead of being weaker, were ftronger then before: I again introduced more mercury, fo that there only remained in F *e* thirty lines that were empty, when I again found that the figns were ftill more vivid; and yet, the air was brought within the tube to a denfity treble to that in the atmofphere.

414. I fufpect that Monf. Du Faye, and the other perfons who have repeated this experiment, condenfated the air in the tube with a fyringe, the plug of which had been foaked with water; at leaft, when I have condenfated the air with a fyringe prepared with oil, I have been able to charge fully, as I mentioned above the glafs-jar inclofed in the tube (Pl. VI. fig. 7.)

SECTION

SECTION III.

On preſſing electricity, or on electric atmoſpheres.

CHAP. I.

On the firſt and principal property of electric atmoſpheres; and, on the other and ſecondary properties of the ſame, which immediately proceed from the former.

415. I. *THE electricity of a body* A *does not ſubſtantially diffuſe itſelf into the ambient air*; that is to ſay, if a body A be electrified by exceſs, the fire added to it does not mix itſelf, at leaſt to any perceivable height, into the ſubſtance of the air around it; and if the body A be electrified by deficiency, the fire drawn from it has not been extracted from the ſubſtance of the ambient air. I think I was the firſt who demonſtrated this fact (Art. Elect. p. 54.) I obſerved, that when I drew a ſpark from a conductor of gilt paſteboard, ſmall ſparks were ſeen to ſhine in all theſe places where the gilding was interrupted. Theſe ſparks proceeded, I conjectured, from the exceſſive fire which, in electrifying this conductor, had by little and little diffuſed itſelf into ſuch naked, reſiſting places; and which, running back afterwards into the neighbouring gilding, at the inſtant when the ſpark was drawn, ſparkled proportionally to the reſiſtance it met; therefore, I added, if the exceſſive fire that is introduced into a conductor, actually penetrates into the ſubſtance of the ambient air to any perceivable diſtance, *it muſt in flowing back to that conductor through that reſiſting, though rare ſubſtance, likewiſe ſparkle.* This conſequence was a neceſſary one; we ſee, lightning emits light whenever, in conſequence of the ſcantineſs of the ſubſtance that affords it a paſſage, it guſhes into the ambient air. A ſtrong ſpark while it paſſes through a ſtripe of gold, ſilver, &c. likewiſe exhibits a light, where the capacity of the metal is too ſcanty. It is a very agreeable ſight to conſider the effects produced by the diſcharge of a

bottle through a chain of iron wire, hanging under a glafs receiver, within which the air has been dilated to an half part of its denfity; this chain appears like a column every where furrounded by extenfive winding flafhes of light; which, befides melting and diffipating particles of the metal where the contact between the rings of the chain takes place, gufhes more amply out of the fame, in proportion as the air around it oppofes lefs refiftance.

416. If the body A were electrified by deficiency, the fame reafoning to hold true needs only to be inverted; the electric fire in its returning into the fubftance of the air, whence it has been extracted, likewife emits light.

417. But the proofs of my propofition are, befides, completed by the following one, the truth of which has been demonftrated by Mr. Canton and Dr. Franklin (Franklin, p. 155.) viz. *The electricity of a body* A, *actuates the ambient air in fuch a manner, that by the means of the fame it tends to introduce into the neighbouring body* B *immerfed in it, an electricity contrary to its own.* I prefent a moft fine flaxen thread to the conductor Y (Pl. I. fig. 1.) and if it be held at the diftance of a foot, or more, from the furface of it, it will direct itfelf perpendicularly to it. I then prefent to this thread a rubbed ftick of fealing-wax, and it flies from the conductor; I prefent to it a rubbed ftick of glafs, it runs again to it; which is the fame as to fay, the thread immerfed in the atmofphere of the body or conductor A, which is electrified by excefs, becomes itfelf electrified by deficiency. I repeat the experiment by the means, not of the Chain, but of the beams B and E of the Machine; then the thread flies from the glafs, and runs to the fealing-wax; whence it is likewife evident, that a body immerfed in the atmofphere of another body electrified by deficiency, will itfelf become electrified by excefs.

418. III. *From the electricity of a body* A, *an electric atmofphere arifes in the ambient air, in the fame manner as, from the electricity which takes place on the one furface of a glafs, a charge tends to take place on both its furfaces.* M. Wilke *, and M. Epino †, were the firft who made

* See his Difputatio Phyfica de Electricitatibus contrariis, Roftock, 1757, p. 77.

† See his Tentamen Theorie Electricitatis & Magnetifmi, p. 97.

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the experiment of inclofing a ftratum of air between two plates of metal, and they obtained a ftroke from it; they obferved, befides, that thofe two plates ftrongly attracted each other, as is the cafe with bodies *contrarily electrified*; that fometimes a fpontaneous difcharge took place, in the fame manner as a vehement fpark fometimes breaks even through the compact fubftance of a charged plate of glafs; that a finger inferted between the two plates effected the difcharge and was ftruck; that the difcharge always took place from fome prominence on the plates; that a point annexed to either of them hindered the charge.

419. But thefe facts do not afford a complete demonftration of the above propofition, and a farther confirmation of its truth may be deemed neceffary. If the charge of a ftratum of air be really a charge, it can be but a very fmall one, and it can fcarcely exceed an ordinary fpark; I have, therefore, fought for a method of examining whether fuch charge has the real characteriftics of a charge, (viz. the accumulation of contrary electricities on the two oppofite furfaces of the faid ftratum) and whether fuch electricities can only be produced and deftroyed jointly. With three filk ftrings (Pl. VI. fig. 10.) I hung two round plates of tin, parallel, and diftant from each other an inch or fomething more; and left the electricity fhould diffipate from their fharp edges, I covered the latter with fealing-wax, and fufpended both plates to the conductor with a fmall iron chain.

420. Then exciting the electricity in the conductor, and the apparatus of the two plates being in the meanwhile infulated, I obferved that two extremely thin threads annexed to *c d*, amply diverged from each other, as did alfo two fimilar threads annexed to A B; and that the former, as well as the latter, were electrified by excefs; as is the cafe with bodies immerfed in atmofpheres impregnated with exceffive fire. I then infulated myfelf (Pl. I. fig. 1.) in fuch a fituation as to have my face placed near the round knob by which the great conductor Y is terminated, and I ftretched my arms parallel to the axis of the fame; in fuch fituation my legs and back became electrified by excefs.

421. Secondly, I drew off all the electricity from the conductor, and when the threads were quite down, there remained in

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me no ſign of electricity. Likewiſe, when I graſped the conductor, being thus inſulated, and having my face and arms near it, all electricity diſappeared on my back and legs.

422. Theſe two facts are the ſame as thoſe obſerved in a bottle ſuſpended to the conductor, and left untouched. The fire from the conductor endeavouring to accumulate itſelf on the inner ſurface of the bottle, drives an equal quantity of fire from the outer ſurface into the coating, and through it into the ambient air; and when the exceſs which endeavoured to condenſate itſelf on the inner ſurface is ſtopped, the natural fire that had been driven from the outer ſurface into the contiguous air, falls back into the ſame. Thus the exceſs from the conductor endeavours, by the means of the upper plate A B, to condenſate itſelf on the ſurface *a b* of the incloſed ſtratum of air, and vibrates an equal quantity from the inferior ſurface of the ſame, into the inferior plate CD *cd*, and through it into the contiguous air *c d:* the exceſs from the conductor being ſtopped, the natural fire falls back from the air contiguous to *c d*, into *c d*, and, through the plate, returns into the inferior ſurface of the air, which ſpreads over C D. The ſame muſt be ſaid of the electricity that riſes in my back and legs, and which, I being inſulated, vaniſhes at the ſame inſtant that the electricity in the conductor is ſuppreſſed.

423. And as thoſe electricities thus raiſed on bodies immerſed and inſulated within electric atmoſpheres, vaniſh as ſoon as the electricity of the body that actuates the latter vaniſhes, they may be called *electricities of ſimple preſſion.*

424. Let us now examine whether electric atmoſpheres produce ſuch electricities as may really be called *charges.* I touch the plate *cd*, and obtain one ſpark from it, and no more; whence I conclude, that if the incloſed ſtratum of air takes a charge, it is but a very ſmall one. In the ſame manner, when I ſtand inſulated, as in No. 420, another perſon can hardly draw from me more than one ſpark; the ſame concluſion as that above is therefore to be drawn with regard to the ſtratum of air intervening between the conductor and my body.

425. Fourthly, I try to diſcharge the ſtratum of air by *alternation.* When I touch A B, I obtain a ſtrong ſpark, but leſs than that

that which I drew when I touched the two plates at once, I then touch *cd*, the ſpark is ſtill leſs; touching AB again, I draw another ſmall ſpark, which I hardly feel with the tip of my noſe; I touch AB a third time, and draw no ſpark, only the threads in *cd* manifeſt ſome inclination to move from each other; I touch *cd* a third time, and I have no ſpark, only the threads in *cd* move as if intending to join. Continuing to touch I can perceive no farther ſigns.

426. *I therefore conclude, that in the ſtratum of air incloſed between the two plates, there is a real, though indeed exceeding ſmall charge;* I ſay a real charge, becauſe the contrary electricities of ſuch ſtratum cannot be ſeparately deſtroyed but by the means of an *alternation*; but this charge is, at the ſame time, very ſmall, becauſe the alternation does not extend, at leaſt ſenſibly, beyond the third contact; and even ſo early as at the ſecond, the ſpark drawn from AB is very weak.

427. Therefore, we ſhall call *electricity from an actual charge* (though a very ſmall one), that which takes place in a body immerſed in an electric atmoſphere, when ſuch body communicates with another body.

428. The fluidity of air, which makes it liable to be traverſed to a certain diſtance by ſparks, prevents the charge of any thin ſtratum of that rare moveable element; and there is, on the other hand, juſt ground to ſuſpect, that the degree of thickneſs which is neceſſary for preventing a ſtratum of air from being traverſed by ſparks, obſtructs its charge, in the ſame manner as we ſee that inſulating bodies are but difficultly and imperfectly charged, when they are thick beyond a certain degree. I intend to procure a large drum of cryſtal, an inch and an half high; I will fit to it as a bottom, a large plate of highly poliſhed metal (ſurfaces thus poliſhed do not throw ſparks but from very ſhort diſtances); as a cover or lid to that drum, I will fix on it a ſimilar thick plate, but perforated in its center, ſo as to receive within its hole a rod of metal, ſo adjuſted as to move in it without giving an entrance to the air; this rod is to raiſe and lower at pleaſure, within the drum; another plate poliſhed in the ſame manner as the bottom of

of it. I moreover propoſe to condenſate the incloſed air, and to examine whether greater charges may be formed by the means of a ſtratum of air thus condenſated: by theſe means, the body of air to be charged may undoubtedly be brought to have a much leſs degree of *thickneſs* than in the before mentioned experiments.

429. Fifthly, and laſtly, I demonſtrate by another method the reality of the charge (though it be ever ſo ſmall) of a ſtratum of air of a ſufficient thickneſs. I take the plate C D *c d* from the apparatus, and fix to it a ſtick of ſealing-wax, by which I hold it; and then preſent it again, thus inſulated, under the other plate A B *a b*, which remains ſuſpended to the Chain; and I ſoon ſee the threads in *c d* diverge and fly from a rubbed ſtick of glaſs; in a word, they are electrified by exceſs. I remove the plate C D *c d*, the threads inſtantly fall, and all electricity in the plate vaniſhes, which is the characteriſtic of an electricity of *ſimple preſſion* (423). I again preſent at a convenient diſtance the plate C D *c d*, under the other A B *a b*, and touching the ſame in *c d*, I inſtantly complete the ſmall charge, and the threads in *c d* fall down and remain in *h*; but I no ſooner remove the plate, than the threads begin to diverge, and fly from a rubbed ſtick of ſealing-wax, and appear electric by deficiency. The cauſe of all this is, that the electricity by deficiency introduced into the ſurface of the air contiguous to C D, remained concealed, as long as it was in a manner retained back by the exceſs in the other ſurface of the ſaid ſtratum contiguous to *a b*; but as ſoon as the plate C D is removed, this deficiency ſuddenly manifeſts itſelf.

430. This, at the ſame time that it demonſtrates to me the reality of the charge of a ſomewhat thick ſtratum of air, confirms alſo to me the conjecture I have propoſed elſewhere, with regard to the charges of inſulating bodies in general, viz. that the quantity and connection of the two electricities that conſtitute a charge, depend on their contrariety to each other, and reciprocal combination on the oppoſite neareſt ſurfaces of the charged body, and do not in any ſhape ariſe from any peculiar (whether poſitive or negative) property in that body. In fact, if ſuch a ſmall electricity *from a charge* as that in the plate C D *c d*, becomes ſo ſenſibly manifeſted when that

plate

plate is removed from the plate A B *a b*, with how much greater a degree of force would not the infinitely ſuperior contrary electricities of a plate of glaſs, become manifeſted, if they could be ſeparated from each other in the ſame manner? Certainly the connection between the two contrary electricities that conſtitute a charge, depend on the above mentioned combination: ſo long as the two tin plates remain near one another, and the two electricities on the two ſurfaces of the cloſed ſtratum of air remain thereby combinated together, theſe electricities cannot be deſtroyed otherwiſe than by an *alternation*; but as ſoon as, by removing the plate C D *c d*, this combination is deſtroyed, either of the two electricities may be ſuppreſſed by a ſingle contact.

431. This idea of conſidering the ſtratum of air *a b* C D (Pl. VI. fig. 10.) as being really charged, naturally leads us to apply the principle of an electricity by *ſimple preſſion*, to any *ſurface of air* contiguous to electrified bodies. Thus, for inſtance, when the electricity of the conductor to which the two plates are ſuſpended, begins to penetrate the plate A B *a b*, the other plate C D *c d* being untouched, it would, in this caſe, be more accurate to ſay, that the exceſs that gets into the plate A B *a b*, tends, it is true, to condenſate itſelf on the ſurface of air contiguous to *a b*, endeavouring, at the ſame time, to drive the natural fire from the ſurface of air contiguous to C D; but that it only can drive the natural fire from the ſurface C D of the plate C D *c d*, which, through the ſame, condenſates itſelf on the air contiguous to *c d*, becauſe the exceſs maintained in *c d* does not allow the natural fire to part from the ſurface of air C D; and, as a neceſſary conſequence, prevents a condenſation of the exceſſive fire on the ſurface *a b*, &c. According to ſuch accurate manner of expreſſing the caſe we indeed are to underſtand what has been related in No. 422; but every body ſees what a confuſion would enſue from a ſtrict obſervance of it; therefore, I beg leave uſually to expreſs myſelf more conciſely, and only to conſider the electricity that takes place in the bodies themſelves that confine electrified ſtrata of air. Conformably to ſuch licence, I ſhall ſimply ſay, that the electric fire which is accumulated in a body, endeavours to drive the natural fire from another body that is not electrified; and

that the natural fire rendered deficient in a body, endeavours to make a correſpondent quantity of exceſſive fire accumulate on the ſurface of another neighbouring body that is not electrified.

432. And availing myſelf of the above ſhortneſs of expreſſion, I ſhall proceed farther, now that I have examined into the firſt property of electric atmoſpheres, viz. *that the electricity of a body endeavours, through the atmoſphere which it actuates, to introduce a contrary electricity into a body immerſed in it:* I ſhall proceed, I ſay, and try to deſcribe the two ſecondary properties, which neceſſarily proceed from the above mentioned.

433. Of theſe the firſt is, that, *when two bodies, impregnated with homologous electricities, meet, theſe electricities, by means of the atmoſphere which they actuate, endeavour reciprocally to deſtroy each other.* The reaſon is, becauſe the electricity of each of theſe two bodies, endeavouring to introduce a contrary one into the other body, will, conſequently, begin with deſtroying the homologous electricity it finds in it; therefore, if ſuch electricities, beſides being homologous, are moreover equal, they will at firſt leſſen, and then, if they are at ſuch diſtance that they can ſufficiently exert their mutual influence, intirely deſtroy each other.

434. If the two electricities are in unequal quantities, the remnant of the more copious electricity will be employed in producing a contrary electricity in that body, the electricity of which has been deſtroyed.

435. The other ſecondary property of electric atmoſpheres is that, *when two bodies impregnated with contrary electricities, meet, theſe electricities, by means of their intervening atmoſpheres, reciprocally increaſe their intenſities.* This is plain; the ſame force which endeavours to produce a contrary electricity, where none exiſts yet, muſt likewiſe endeavour to increaſe the ſame when it already obtains.

436. But let us return to the principal property, and expreſs it with more exactneſs, applying it to the two electricities by exceſs, and by deficiency; and let us examine whether from ſuch exactneſs we may derive ſome leſs abſtracted idea, and that may be repreſented by ſome ſenſible image. The aforeſaid principal property is, therefore, to be expreſſed thus, with reſpect to an elec-

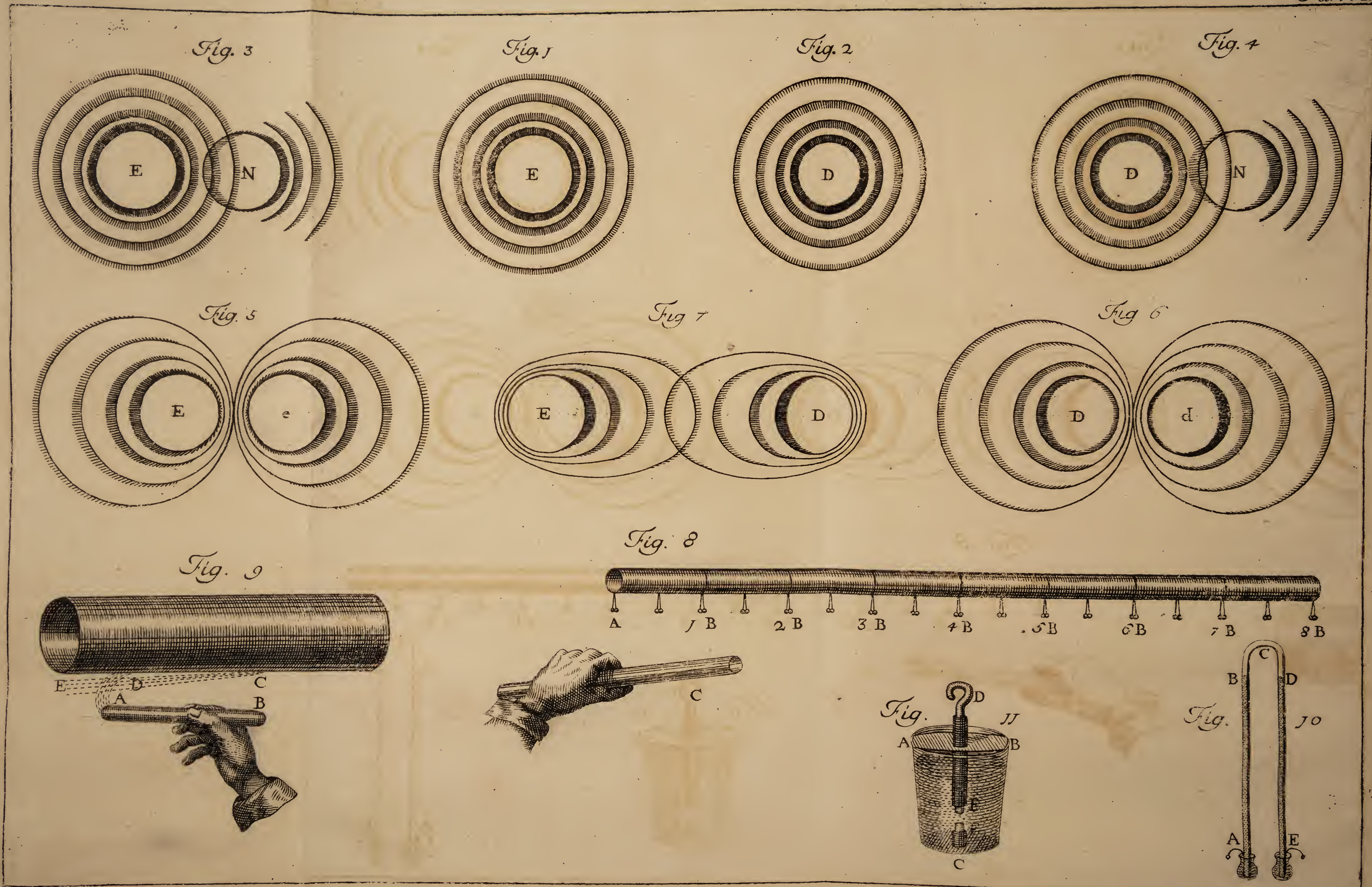
Fig. 3
Fig. 1
Fig. 2
Fig. 4
E
N
E
D
D
N
Fig. 5
Fig 7
Fig 6
E
e
E
D
D
d
Fig. 8
A
1 B
2 B
3 B
4 B
5 B
6 B
7 B
8 B
Fig. 9
E
D
C
A
B
C
Fig. 11
D
A
B
E
C
Fig. 10
C
B
D
A
E

electricity by excefs; *if a body poffeffes any portion of exceffive fire, the latter will actuate the ambient air in fuch a manner, that it will endeavour to drive the natural fire from fuch bodies as are immerfed in it.* Now the exiftence of an excefs of electric fire implies a greater denfity of it, a greater expanfive force in it, a greater tenfion of the fame, whatever it may be; and muft not fuch tenfion introduce a greater tenfion alfo into the ambient air, and cannot we confider fuch tenfion thus ordinately propagated through the fire inherent in the fucceffive parts of the ambient air, as ferving to vibrate the natural fire from the air immerfed in it? Therefore, if a body E, be electrified by excefs (Pl. VIII. fig. 1.) firft a fet of fmall lines fpringing againft the ambient air from all points of the furface of the faid body E (we fhall fee hereafter that both the excefs and deficiency of electrified bodies only take place on their furface) may very well reprefent both the exceffive fire of fuch body, and the increafed tenfion exerted by the fame fire againft the natural fire of the ambient air; fecondly, the ambient air may likewife be underftood as being divided into fucceffive exceedingly thin ftrata, and fimilar fmall lines may alfo, being underftood to be applied to the convex furface of all thofe ftrata, exprefs the direction and manner in which the exceffive tenfion of the fire on the furface of that body, propagates, &c.

437. If we apply to cafes of deficient or negative electricity, the above principles concerning the firft property of electric atmofpheres, we ought to fay, *if a quantity of natural fire be taken from a body, the air around it will from thence be fo actuated, that a quantity of exceffive fire will run to another body immerfed in that air*, &c.

The Italian Author, in the two foregoing paragraphs, and in the three following ones, attempts to defcribe mechanically the action of electrified bodies upon the air around them, and the action which that air, or atmofphere, in its turn exercifes on fuch bodies as are immerfed in it. In thefe mechanical defcriptions, or explanations, the Author is greatly affifted by the diminutive expreffions with which his own language fupplies him; but the want of fuch expreffions in Englifh, together with the great difficulty that always attends abftracted defcriptions, or rather conjectural reafonings, of this kind, has induced us to pafs over

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the ſaid paragraphs, and to go to the 441, *by which this chapter is terminated. In this paragraph our Author ſays, that in caſe it ſhould be found that in the mechanical deſcription he has juſt attempted to give of the manner in which electric atmoſpheres both are actuated, and act, he has too much indulged his own imagination, as well as applied to that of his readers, he now is going to make amends for it, by elucidating the properties of thoſe atmoſpheres from a number of accurate experiments.*

CHAP. II.

In which the three above properties of electric atmoſpheres are farther demonſtrated, by experiments chiefly made within cavities in deferent bodies. The ſuperficiality *of the electricity of deferent electrified bodies is demonſtrated. And a few conjectures are offered to the reader concerning the proportional quantity of the electric fire within the ſubſtance of deferent, and of inſulating, Bodies.*

442. I Call *electric well* (Pl. VII. fig. 1.) the veſſel, or cann A, made of tin, fifteen Italian inches high, and ſix and an half wide; and, to prevent its loſing its electricity, its edge has been rounded by the means of an iron ring fixed to it. I inſulate this cann, or *electric well*, upon a ſmall table T, raiſed upon a ſupport of glaſs V, and I commonly electrify it by touching it with the hook (or with the coated bottom) of a charged bottle, which I hold by the means of an inſulated handle (Pl. III. fig. 3.): I need not ſay, that when I touch the *well* with the hook of the charged bottle, I obſerve to touch with my finger the outſide of the ſame, and vice verſa, to touch the hook with my finger, when I mean to charge the well with the outſide of the bottle. In this well I diſtinguiſh two parts; the firſt is the *lower* part of it, that is, that part of its cavity which reaches from the bottom, to two third-parts of the total altitude of it; the *upper* part is that which, from thence, extends up to the edge of the well.

443. I call *ſcrutator* an electroſcope annexed to a long ſtick of ſealing-wax (Pl. VII. fig. 1.), the threads of it are exceedingly fine,

fine, and only an inch and an half long, and to them are faftened two bits of paper in order to render their motions within the cavity of the well fufficiently confpicuous; and that I may perceive them the better, I cover the bottom of the well with a round plate of tin blackened over.

444. I. A man fufpends the fcrutator in the middle of the lower cavity of the well, in fuch a manner that it does not touch either the bottom or the fides; I then touch the well, at one time with the hook, and at another with the outfide of the bottle; when I find that the threads remain unmoved. II. The perfon who holds in his hand the threads of the fcrutator now touches the bottom, and then the fides of the lower cavity, and the threads ftill remain unmoved. III. The fame perfon fufpends again the fcrutator in the middle of the lower cavity, without touching either the fides or bottom of the well; I then infert into the well a fmall rod of brafs, with a ball at its end, and prefent it to the threads of the fcrutator, taking care not to touch either the edge or fides of the well, and the threads fly to the ball. I deftroy the electricity, and the perfon who holds the fcrutator draws it out of the well; then the threads manifeft an electricity contrary to that which I had communicated to the well; that is, if I touched the well with the hook, the fcrutator runs to the hook, and flies from the outfide of the bottle; if I touched with the outfide of the bottle, the fcrutator flies to the outfide of the bottle, and runs from the hook. IV. I annex a fhort and very fine thread laterally to the lower cavity of the well; the fcrutator is again fufpended within it, and I, with the bottle (I always underftand it to be ftrongly charged) electrify the well. Seeing that both the threads of the fcrutator, and the annexed thread remain unmoved, I put the brafs rod into the well, and prefent it to the threads of the fcrutator, when the latter inftantly run to the rod, and the annexed thread diverges a little from the well; if both the annexed thread and thofe of the fcrutator happen to be near each other, they immediately join.

445. I fhall relate another experiment on the fame fubject, the confequences of which are ftill more obvious than thofe of that juft defcribed. I faften to three filk threads B, a cylinder of tin

(Pl.

(Pl. VII. fig. 2.) two inches high, and alſo two inches wide, the edges of its baſis are rounded, that it may not attract electricity, and I call ſuch cylinder the electric bucket. I. A man ſuſpends this bucket within the lower cavity of the well, taking care that it does not touch either the ſides or bottom of it; I electrify the well, the man draws the bucket out, taking care to keep it at an equal diſtance from the ſides; I then preſent a thread to it, and it is not moved in the leaſt. The man puts again the bucket into the lower cavity of the well, of which I revive the electricity; he draws it off again; I then preſent a thread to it, and it ſtill keeps unmoved. III. The man again puts the bucket into the lower cavity of the well; I electrify the well, and then put into it the rod C, the ball of which I preſent to the bucket; then a ſpark is thrown out of it, which, with reſpect to its exploſion and light, is much ſuperior to what might be expected from the capacity of the bucket. The man again draws the bucket out, I preſent the ſcrutator to it, and the threads are rapidly drawn, then repelled by it. When I have touched the well with the hook of the bottle, the threads repelled by the bucket, are alſo repelled by the outſide of the bottle, but drawn by the hook; when I touched the well with the outſide of the bottle, the threads repelled by the bucket, are alſo repelled by the hook, but drawn by the outſide of the bottle. IV. The man ſuſpends within the well two buckets of an equal ſize D, *d* (Pl. VII. fig. 3.), the bucket *d* hanging from the bucket D, by ſilk threads two inches long: the bucket *d* is lowered till it actually touches the bottom of the well; both are drawn out, and neither of them can move the threads I preſent to them. V. The man again puts the buckets into the well; the lower *d* touches, as it did before, the bottom of it, which D does not; I then touch D with the rod, and a ſtrong ſpark is thrown out; the man draws out the two buckets, when both draw the threads, but D does it moſt ſtrongly, and *d* but very weakly. VI. The experiment is repeated, and the buckets are drawn out; I preſent to D the threads of the electroſcope, after they have touched the part of the bottle contrary to that with which I electrified the well, and they are repelled; I then with my finger touch the bucket D, and theſe ſame threads which are ſtrongly repelled by D,

D, are weakly drawn by *d*. VII. I again repeat the experiment: the buckets being drawn out, I fuddenly deftroy the electricity in D, and the threads of the fcrutator, which have touched that coating of the bottle with which I have electrified the well, are repelled from *d*.

446. Dr. Franklin, in page 325 of his works, propofes his experiment of a cork ball, which, hanging by a filk thread, and lowered into a filver cann till it touches the bottom of it, draws no electricity from it. I was informed of this experiment by a fhort but ingenious differtation of M. de Sauffure, a philofopher of Geneva, when, in February 1769, I publifhed my little book *de Atmofpherâ Electricâ*, in which I fully analyfed this furprifing and myfterious experiment. I have fince received the Work of Dr. Prieftley, publifhed in the year 1767, in which I have feen that the ingenious author had made feveral attempts (page 731.) to analyfe the fame experiment, though he had nowife fucceeded; this I attribute only to the extreme delicacy of the fubject, which requires both a moft favourable weather, and a moft careful management throughout the whole experiment. The weather being fuppofed favourable, here are a few things amongft others, that muft be attended to: I. The bottle with which the well is to be electrified, muft, inftead of a hook at the end of the rod inferted into it, have a pretty big ball fixed to it, becaufe the hook which is commonly ufed, being made of a thin rod terminating in a point, may eafily turn the electricity in the threads of the fcrutator into a contrary one. II. When afterwards the electricity is to be excited in the threads of the fcrutator, they muft at the fame time that they are prefented to the outfide of the bottle, or to the hook (I retain the fame name though a ball is now ufed), be rapidly touched with a finger, that they may thereby, firft, contract a contrary electricity, and then fly to thofe bodies and impregnate themfelves with their electricity. III. Before drawing either the fcrutator, or the buckets, out of the well, the electricity of the latter muft be deftroyed, left thofe bodies in their paffing through the mouth of it, fhould receive an alteration in their electrical ftate, from the united atmofpheres of the edge. IV. But the chief reafon why, when the two buckets are drawn at once, it

becomes

becomes neceſſary to ſuppreſs the electricity of the well, is to prevent a mixture of the two different electricities; that is, the ſtrong one in D, and the weak one in *d*: it is needleſs to obſerve, that before ſuppreſſing the electricity of the well, the bucket *d* muſt be previouſly raiſed ſomewhat above the bottom, elſe its electricity would be deſtroyed alſo. V. Moreover, when the two buckets have been jointly drawn out, it muſt be obſerved, that as long as D will retain the electricity it has contracted within the well, the bucket *d*, which will be ſurrounded by the atmoſphere of D, will repel the threads of the ſcrutator when they have been repelled by D; therefore, the electricity of D muſt be previouſly deſtroyed to inquire after the contrary, and weaker one in *d*.

447. Theſe cautions being once carefully attended to, the experiments that I have deſcribed are always followed by the ſame effects, and afford an analyſis of the ſaid fine Franklinian experiment, as well as complete the demonſtration of the property of electric atmoſpheres, which I deduced in the precedent Chapter from the ſimple experiment of a thread preſented either to the Chain, or to the Machine: the above experiments, beſides, open a vaſt field for intereſting and important diſcoveries.

448. All theſe experiments concur in making it manifeſt, *that the electricity introduced into the well, endeavours, it is true, to excite a contrary one in the threads of the ſcrutator, or in the bucket, or in any other body placed within its cavity* (in which conſiſts the firſt property of electric atmoſpheres.) II. *But that the homologous electricities that ſeek to communicate themſelves to the above bodies from oppoſite parts of the cavity, reciprocally obſtruct and annihilate each other* (which is the ſecond property), as long as thoſe immerſed bodies have no communication either with the ground, or other external body, by the means of which they may be enabled to contract an electricity contrary to that in the well.

449. Secondly, from the ſame experiments, a few additional truths are moreover diſcovered, concerning the above properties of electric atmoſpheres; for inſtance, they ſhew *that an exceſs of fire endeavours to produce a deficiency equal to it, and vice verſâ.* If this were not the caſe, how could it happen that the electricity of the

the bucket *d*, which touches the bottom of the well, comes out of the ſame with an electricity ſo inferior to that of the bucket D, which remains ſuſpended within the cavity of it? ſurely this muſt be imputed to the aforeſaid equality. From the inner ſurface of the well, no quantity of fire can be exerted greater than that which can be driven from the ſmall ſurface of the bucket D; or, if the well be negatively electrified, from the inner ſurface of the well, no quantity of natural fire can paſs to the outer ſurface of the ſame, greater than that of the exceſſive fire which can flow to the inconſiderable ſurface of the bucket D; therefore, as the whole capacity of the inner ſurface of the well, is to the capacity of the bucket *d*, ſo the intenſity of the *excited* electricity (whether poſitive or negative, which all lies in D) is to the intenſity of the electricity in the bucket *d*, into which only a proportional part of the *exciting* electricity is diffuſed.

450. Fourthly, from the ſame experiments we may again draw this conſequence, viz. that *the electricity which ariſes in the bucket immerſed within the cavity of the well, muſt be as much ſuperior to the electricity that would take place on the ſame, if immerſed in the outer atmoſphere of the well, as the number of points in the cavity that concur to actuate a contrary electricity in the bucket, is greater than the number of points that endeavour to introduce a contrary electricity into the ſame, when ſuſpended in the outer atmoſphere.* In fact, the bucket outwardly ſuſpended gives a ſpark much weaker than that which is thrown out by it, when it is placed within the cavity of the well, &c.

451. In the third place, it follows from the ſame experiments, *that, both the quantity of electricity that will be exerted from the cavity of the Well and the atmoſphere that will be excited by it, as well as the contrary electricity that will riſe in that part of a body which is only partly immerſed in the Well, will all of them be in proportion to the capacity of that part of the ſame body which is not immerſed.* With three ſilk ſtrings joined together in B (Pl. VII. fig. 4.) I ſuſpend within the well A, the cylinder C, the edge of which is rounded at its baſis, and I keep it at an equal diſtance, that is two inches, both from the ſides and the bottom. I electrify the well, and that part of the cylinder C, which is not immerſed, repels

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the ſame electroſcope that is repelled by the well; it poſſeſſes, therefore, the ſame kind of electricity. This may be eaſily explained: the electricity of the well, by raiſing an electricity contrary to its own in the immerſed part of the cylinder, neceſſarily introduces a contrary one (conſequently one homologous to its own) into that part which is not immerſed; or, in other words, the natural fire driven from the former part of the cylinder goes into the latter; or, if the well be negatively electrified, the natural fire runs from the part which is not immerſed and accumulates itſelf on that part which is immerſed. Therefore, according as the capacity of that part of the cylinder C which remains out of the well in the open air (it muſt be obſerved, that the air near the mouth of the well is ſomewhat affected by the atmoſphere, both of the edges, and of the upper part of the cavity, which obliquely exerts itſelf) will be greater or leſs, both the excited and the exciting electricities, and the atmoſphere through which this excitation is effected, will alſo be proportionably ſtronger or weaker.

452. From the ſame experiments it reſults alſo, *that electric atmoſpheres exert themſelves and operate obliquely*; this I have juſt now taken for granted, I will now try to demonſtrate it. Let us for an inſtant ſuppoſe that electric atmoſpheres only operate directly; if ſo, the electricity from the bottom of the well will ſuffer no kind of counteraction, while the ſides of it will counteract each other; an electricity will, therefore, be able to ſpring directly from the bottom of the well, and none will be thrown from the inner edges, ſince every point of them has ſome other point directly oppoſite to it; but now, this is not by any means the caſe; we muſt, therefore, acknowledge, that the electricity that is thrown from the bottom is counteracted by that which obliquely ſprings from the ſides of the well, directing itſelf towards that ſame bottom; and alſo, that a certain quantity of electricity really exerts itſelf in an oblique direction, from the inward edge or mouth of the well. All this may be very eaſily aſcertained by the following experiment. Let a number of ſhort threads, about an inch and half long, be, both outwardly and inwardly, annexed to the ſides of the well; let them be diſpoſed in a vertical line, ſo as

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to form as it were a ſingle continued thread. When the well is electrified, thoſe threads which ſtand within the lower cavity of the well, keep their former ſituation; thoſe which are adapted to the outſide of it, are all raiſed into lines perpendicular to the ſides of the well, though with a ſmall deviation from each other, by which the one are inclining to the right, the other to the left ſide alternately: thoſe which are annexed to the upper part of the inſide of the well, diverge more or leſs from it, according as they ſtand nearer to the mouth; and if any threads have been annexed to the bottom of the well, they remain entirely unmoved.

453. This oblique action of electric atmoſpheres explains how it comes to paſs, *that the atmoſphere of the one ſurface of an inſulating plate, extends itſelf ſo far as to reach the other ſurface of it*; and from thence comes to be completely underſtood, one of the moſt perplexing experiments that occur in the ſcience of electricity. The experiment has been firſt made by Mr. Richman: M. Epino, as I underſtand from Mr. Prieſtley's Work, has related it, but as I have not been able as yet to procure his book, I ſhall deſcribe the above experiment in the manner that it has conſtantly ſucceeded with me, and conformably to the analyſis I have made of it. Let A B (Pl. VI. fig. 13.) be a glaſs plate nine inches long, and ſeven inches wide; let M N and O P repreſent its coatings; C D, E F, are two very thin threads, an inch and an half long, with ſtripes of paper hanging from them, and they are annexed to the middle points of both coatings; I then inſulate the plate on a ſtick of ſealing-wax, and make the coating M N communicate with the Chain, though in ſuch a ſituation that the electricity from the Chain do not counteract the electricity which is to riſe in the thread C D. From the ground I touch the other coating P O, taking care to keep my finger at ſuch a diſtance O from the thread E F, that it may not influence its divergence; and then I obſerve, I. That in proportion as the charge is farther promoted, both threads C D, E F, gradually acquire a greater divergency. II. That if, in this ſtate of things, a perſon inſulated and communicating with the Chain, preſents the palm of his hand parallel to the plate, near the thread C F, the latter falls; and if from the ground I likewiſe preſent my hand to the thread E F (ſtill touching the coating in O), the thread E falls likewiſe. If both the hands that are preſented to

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the threads, or either of them, are removed, both the threads, or either of them, begin again to diverge. III. If, when the charge is completed, I prefent my hand to the thread E F, it falls down, though I entirely leave off touching the coating with my other hand; but, on the contrary, if before the charge is completed, I prefent my finger to the thread, and leave off touching the coating, it flies to my finger.

454. All the difficulty arifing from this experiment is refolved by fuppofing (which is the truth), that the electric atmofphere, which (while the charge is advancing) is excited from the coating M N, bends its courfe, and actually extends itfelf to the air contiguous to the other furface of the plate; hence both the coating P O, and the thread E F, being immerfed in air actuated by the electric atmofphere from the oppofite furface which is pofitively electrified, become electric by deficiency; therefore, the thread E F diverges from the coating. But when afterwards I oppofe my hand to this thread E F, my hand alfo becomes electric by deficiency; hence the homologous electricities of the coating P O, and of my hand, oppofing the action of each other, the thread between them falls. When I prefent my hand to the thread E F, and ceafe at the fame time to touch the coating, then the thread E F runs to my hand; the reafon of this is, that the natural fire which is continually driving from the coating to which the thread is annexed, runs, the other communication being now removed, to my hand through that fame thread. But when the charge comes afterwards to be completed, as no fire is any longer driven from P O, then, even though I ceafe to touch the coating P O, the thread E F, the coating itfelf, and my hand, all remain electric by deficiency, though the continued action of the electric atmofphere of the other furface M N, which, as has been obferved above, continues to bend its courfe, and to flow to this furface P O.

455. I demonftrate the truth of the above explanation, by experimenting with the large plate A B C D (Pl. II. fig. 8.) I annex two ftripes of gold leaf to the margin of its inferior coating, and place them parallel, and near to each other; I annex two other ftripes to the fame coating, and place them fomewhat more advanced within the compafs of it; and two others befides, at a ftill greater diftance from the margin; this inferior coating is then

then made to communicate with the ground, and the upper coating with the Chain; when I obſerve, I. That in proportion as the charge advances, the electroſcopes, or ſtripes, annexed to the inferior coating, gradually acquire ſome divergence; but with this difference between them, that thoſe which ſtand neareſt to the margin of the coating, manifeſt firſt their divergence; then thoſe which follow them; and, laſt of all, the third ſtripes begin to manifeſt what ſmall degree of divergence they may have acquired. II. The charge being completed, the greateſt divergence ſtill continues to take place in thoſe ſtripes which ſtand neareſt to the margin; a leſs divergence obtains in thoſe ſtripes which have been placed in the middle; and the leaſt divergence is manifeſted by the remoteſt ſtripes. III. All thoſe ſtripes fly from my finger, and thoſe which diverge leaſt, increaſe their divergence when I place my little finger between them. IV. All fly from the outſide of a bottle inwardly charged by exceſs. From theſe facts I therefore conclude, that all the ſtripes are electric by deficiency, as alſo is my finger (though no doubt in a leſs degree than the above mentioned bottle). The reaſon is, that both they and my finger are immerſed in an atmoſphere actuated from an exceſſive electricity, which, bending its courſe, flows to the inferior coating to which thoſe ſtripes are annexed, and introduces a greater or leſs degree of electricity into them, according as they are ſituated nearer to, or farther from, the edge of the plate. That a body immerſed in a given atmoſphere acquires an electricity contrary to that by which this atmoſphere is actuated, is what I think I have demonſtrated before, and what every body may eaſily aſcertain. Preſent a very fine thread to the Chain, and when it begins to move towards it, preſent your finger laterally to this thread, it will fly from your finger.

455. Fifthly, We may conclude from the above experiments, that *from hollow ſurfaces imperfectly cloſed, an electricity will be manifeſted, which will be leſs in proportion as thoſe ſurfaces are nearer being completely cloſed; and in proportion as they change either to plane, or to convex, or to more convex, ſurfaces, an electricity will be manifeſted around them in greater plenty and augmented force*; this is, becauſe in proportion as the ſurface will be leſs hollow and cloſed, and will approach nearer to being plane, the reciprocal counteraction of the atmo-

atmoſphere actuated from different points of this ſurface, will leſſen. But as atmoſpheres, as we have juſt now obſerved, act alſo very obliquely, and even ſo much ſo as to bend their courſe from the one ſide of a plate to the other, always preſerving their property of exciting an electricity contrary to that by which they were actuated in the beginning, it hence follows that electrical atmoſpheres from plane ſurfaces, counteract each other in a leſs degree than thoſe which ſpring from concave ſurfaces, though in a greater degree than thoſe which ſpring from convex ſurfaces; ſo that an electricity that ruſhes from an infinitely convex ſurface, that is, from a very ſharp point, placed at a great diſtance from any other ſurface that may be animated with an electricity homologous to its own, will be counteracted the leaſt poſſible, will be thrown out with the greateſt force, and will exhibit either the bruſh or the little ſtar. But of theſe important concluſions, I only mean in this place to drop a ſeed, or hint; let us for the preſent proceed to reap other fruits from our analyſis of the electric well *.

456. Of ſuch fruits or diſcoveries, the following is certainly a very valuable one, viz. *that we have at length aſcertained that, in* preſſing *electricity, any exceſs of fire, any deficiency of the ſame, any kind of electricity in ſhort, is confined to the free open ſurface of bodies, without in the leaſt diffuſing itſelf into their ſubſtance.* I ſay in *preſſing* electricity, becauſe with reſpect to the *vivid* electricity of a ſpark, there is no doubt but the latter condenſates itſelf for an inſtant within the pores of bodies, and endeavours to break the coheſion of their ſolid parts. With regard to common, *preſſing*, electricity, if no electricity can by it be made to adhere to the wide cavity of an electric *well*, how can we imagine that any electricity will better be brought to adhere and be accumulated within the narrow cavities of the inward pores of bodies? We cannot, it is true, ſuppoſe that within the ſubſtance of bodies there is any ſuch medium as air, by means of which oppoſite atmoſpheres may

* If the reader entertains ſtill any doubt about the reality of the *oblique* notion of electric atmoſpheres, the following experiment will remove them. Let him place one of his hands open under the great conductor, with a thread hanging from the middle of it; he will find that this thread becomes electrified by deficiency, through the *oblique* action of the atmoſphere of the conductor, as well as a finger of his other hand, expoſed to the direct action of this atmoſphere; and, in fact, if he preſents that finger to the thread, the latter will fly from it.

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be actuated; but yet, do not the ſolid particles that conſtitute the partitions between the pores of ſuch compact bodies, for inſtance gold, ſtand much cloſer to each other than do the ſolid particles of air? Muſt not, therefore, the ſame counteraction and oppoſition take place between the portions of electric fire diffuſed within the pores of a piece of gold, as obtains, by means of the fire inherent in the ſubſtance of the air, between the homologous electricities that endeavour to accumulate themſelves within the cavity of the well?

457. On this occaſion, I ſhall remind the reader of the obſervations formerly related in page 76 of *Artificial Electricity*. A cube made only of gilt paper both attracts and gives ſparks with the ſame degree of force as a ſimilar cube of ſolid iron. Here too comes in its proper place the experiment of Dr. Franklin, who ſaw an electricity exerted on the ſurface of a metallic Chain, grow more vivid in proportion as a greater portion of the ſaid Chain was gradually made to gather within a tumbler. I have imitated the ſame experiment after another manner. I adapt fine and ſhort threads to the body of a Man, to his forehead, to his back, to his legs, to his arms, &c. I bid him ſtand, with his arms and legs ſtretched as much as he can, on two inſulating ſtools, and then give him a ſpark from a bottle ſtrongly charged; when I find that the threads immediately diverge; though moſt of all, thoſe which are leaſt counteracted by the electricity of other adjacent parts. The Man afterwards joins together both his legs on one ſtool, drops his arms, and at laſt gathers and folds his body in the ſame manner as taylors uſe to do; then the threads placed in thoſe parts which are become cloſe to each other, fall down; and thoſe, on the contrary, which remain expoſed to the free open air, increaſe their divergence. I repeat the experiment in a contrary manner, and give a ſpark to the Man when he is thus gathered; and as ſoon as he begins to unfold and ſtretch himſelf, the divergence of the threads in the open air leſſens, and thoſe ſituated between joined parts of his body, acquire a divergence, according as they gradually become ſeparated from each other.

458. If to theſe obſervations we add, that any ſpark, even the ſmalleſt, given to, or drawn from, a large conductor, always is ſeen to cauſe a ſenſible exceſs or deficiency in the threads, though

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ever ſo thin and comparatively ſmall, that may be annexed to it; that any electricity excited in one part of an extenſive conductor by any kind of electric atmoſphere, becomes manifeſted on the whole part of that conductor which is immerſed in that atmoſphere, and excites an equal, but contrary one, on that part of the ſame which is not immerſed, then, we ſhall be convinced, that Electricity does not diffuſe itſelf into the ſubſtance of Bodies; or, in other words, that the electrical fire is not condenſated or dilated within the inward ſubſtance of electrified Bodies.

459. But, laſtly, if electricity did really penetrate into the inward ſubſtance of electrified bodies, ſurely when I lower the bucket into the electrified well, and make it touch the bottom, a part of the electricity which, in the caſe we ſuppoſe, is diffuſed within the cavity of it, would alſo diffuſe itſelf into the bucket, and be manifeſted on it when I extract it from the well; now, when the operation is carefully made, the bucket conſtantly comes out of the well without the leaſt electricity.

460. Laſtly, The ſame experiments explain the following paradox, viz. *that the natural quantity of fire contained within inſulating bodies, is incredibly greater than the quantity contained in deferent bodies.* This principle becomes farther confirmed, when we conſider, that an hundred inſulated Men can ſcarcely ſupply the ſurface of a bottle with the neceſſary fire to charge it; neither could they receive the whole quantity that eſcapes from the outſide of the ſame bottle. The ſame thing obtains alſo with regard to the diſcharging of the bottle; an inſulated Man touches an hundred different times the coating of a charged bottle, the other coating of which communicates with the ground; and, though after every touch he takes care to diſſipate the fire he has received (or to recover that which he has given), he never has done exciting freſh ſparks from the bottle; whence I was induced to conclude that the bottle could give or receive more fire than an hundred Men could receive from, or give to, it; and, conſequently, that the natural quantity of fire in an ordinary glaſs-bottle exceeded that contained in an hundred Men.

461. But though we have demonſtrated this truth, that the natural quantity of electric fire remains *unaltered* within the ſubſtance of *deferent* bodies, yet we have nowiſe demonſtrated this,

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viz. that glaſs actually contains more fire than deferent bodies do. A man, for aught we know, may contain a very great quantity of electric fire; this only is aſcertained, that he can give to the bottle no other fire than what he can himſelf receive from other bodies, or receive from the ſame no more fire than he can tranſmit to other bodies. With reſpect to the air around him, as the *charge* of that element can be but very ſmall, a man needs only receive a very ſmall quantity of electricity from it, to exhauſt it; therefore, in this caſe likewiſe, he may actually contain a very great quantity of fire, though he only gives to the air around him, or draws from the ſame, an exceeding ſmall quantity. In order to be able to draw a great quantity of fire from the air, or give it to the ſame, a man muſt communicate with an extenſive ſyſtem, from which he may draw, or to which he may tranſmit, ſuch fire.

462. The above is the eſſential difference between *inſulating* and *deferent* bodies. With reſpect to the former, ſuch is the conſequence of their electrical *impenetrability*, that great alterations in their electric fire may be introduced in their oppoſite ſurfaces; and ſuch alterations are both equal to each other on the reſpective ſurfaces on which they take place, and contrary; but in *deferent* bodies, no ſuch alteration can be effected, on account of their *penetrability*; though, for aught we know, the denſity of the fire in deferent bodies may very well be equal to the natural denſity of it in inſulating bodies.

463. I attempted to aſcertain this by an immediate experiment; I inſulated a wax candle a foot high, ten lines thick, on a glaſs candleſtick; I inſulated a tube of glaſs above the candle, ſo that the flame entered it; above the ſaid tube I again inſulated, with its head downwards, an alembic of braſs, the neck of which was bent backwards and forwards ſeveral times; yet, out of ſeveral very fine threads which I annexed to that neck, none manifeſted the leaſt electricity, either when the glaſs tube became deferent by its being heated till it grew red-hot, or when the ſame afterwards cooled, and thereby recovered its inſulating nature.

464. But, of all others, the following obſervation convinces me moſt of the above truth. A ſtrong ſpark from an electric bat-

tery will vitrify metallic particles, and thus in an inftant from *deferent* render them *infulating*; yet no part of the fpark is found afterwards to have remained affixed to thefe bodies; for the deficiency that may have remained on the one fide of the battery, after a difcharge of the kind we mention, is found to be precifely equal to the remnant of excefs left on the oppofite fide.

CHAP. III.

In which the three properties of electric atmofpheres are confirmed by experiments made on convex bodies. Their power of exciting atmofpheres of a contrary kind on the fame body, is explained; and the point where an equilibrium between thofe atmofpheres takes place, is determined.

465. THE atmofpheres of infulating bodies are in one fenfe immoveable, as well as their electricities; that is, as long as the deficiency or excefs of the electric fire, on given points of fticks of fealing-wax, or glafs, remain unaltered, the air around them will remain actuated in the fame manner; fo that the fire inherent in them will remain relaxed, contiguoufly to the deficient wax, or poffeffed of a fuperior tenfion, contiguoufly to the exuberant glafs; but, in deferent bodies, the electric fire being able to be transfufed from the one part of them to the other, the electric atmofpheres of fuch bodies are moveable, as well as the electricities by which they are produced.

466. Now, in conformity to fuch principle, fince the atmofphere of an infulating body never is difplaced in confequence of its meeting with the electric fire of a deferent body, it follows, that all difplacement that may arife from their mutual action, will take place in the fire of the deferent body; exactly in the fame manner as if an elaftic finite body ftrikes againft another elaftic body, the mafs of which is infinite, the infinite refiftance of this latter will reject into the former all the motion that may arife from fuch a fhock.

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467. In confidering the *electric* alterations that may take place on a deferent body, two parts muft be diftinguifhed on the furface of the fame, viz. the immerfed part, that is to fay, that part of its furface which is immerfed in the atmofphere of the infulating body placed near it; and the part not immerfed, that is to fay, that part to which this atmofphere does not reach. The alterations that will take place on thefe two parts will be contrary to each other in their *quality*; becaufe they will refult from a quantity of fire being given (and as it were lent) by the one to the other. With regard to the intenfity of fuch alterations, they will be in a reciprocal ratio to the capacities of the parts on which they refpectively take place; fo that the deficiency that will arife on that part which will have given its own fire to the other, will be to the excefs that will arife on that other part, in the fame ratio as the capacity of the latter ftands to the capacity of the former. But this principle cannot be thoroughly well underftood, till a few additional experiments have been related and defcribed.

468. Thefe experiments I chiefly make with the following apparatus. In a place fufficiently roomy, I infulate four large tubes, or conductors of tin, two inches and a quarter in diameter, the edges of which are coated at their openings with fealing-wax, in order to prevent their drawing, or throwing any electricity; the firft tube is eight feet long, the fecond four feet, the third two feet, and the laft only one foot. I have reprefented all thefe tubes in one figure (Pl. VIII. fig. 8.) though they were, in my experiments, feparated from each other, and they are infulated by filk ftrings. The furfaces of thofe conductors are divided by marks into inches, and from every divifion hangs an electrofcope (in the figure they are only expreffed at every interval of fix inches to avoid a confufion); each of thofe electrofcopes is made with two very thin threads, two inches and an half long, and two very fmall balls of pith of elder: thofe electrofcopes are fo difpofed, that the threads hang parallel to each other, and the balls almoft touch one another; as the leaft motion, or fhake, will entangle the threads together, care muft be taken before beginning the experiment, to feparate them.

 496. I

496. I prefent the tube of glafs, ftrongly electrified, to one extremity of the conductor A and B, and flowly carry it round the fame at the diftance of half a foot (a diftance through which in dry weather its actuating electricity cannot be communicated to the conductor, and of courfe loft), and I take care to keep this electrified tube under the firft electrofcope A, perpendicularly to the axis of the conductor A. I. All the electrofcopes between A and 2 B, diverge; the electrofcope A moft of all, and the others gradually lefs. The electrofcopes between 2 B and 3 B fcarcely move. From 3 B to 4 B the electrofcopes are alfo found to diverge, thofe near 3 B diverge lefs than thofe near 4 B. The other electrofcopes between 4 B and 8 B diverge all of them almoft equally, but fenfibly lefs than the firft electrofcopes in A. II. The electrofcopes in the whole fpace A 2 B, give figns of a *deficient* electricity; they fly from rubbed fealing-wax. The electrofcopes from 3 B to 8 B, give figns of an *exceffive* electricity; they fly from rubbed glafs, or from the hook of a charged bottle. III. I remove the electrified tube, all the electrofcopes fall, and there remains no fign of electricity in any part of the whole conductor.

470. I propofe to explain at fome length this firft experiment, it will greatly facilitate the underftanding of the fubfequent ones. I. The exceffive fire, which continues actuated on the furface of the electrified glafs-tube C, actuates an atmofphere in the air around it; it excites a particular kind of tenfion, or force, in that air, and by its means endeavours to raife a *deficient* electricity on the whole part of the conductor which the faid atmofphere furrounds; that is to fay, this atmofphere endeavours to drive into the fubftance of the conductor the natural fire which lies near its furface. II. This power in the atmofphere of the glafs-tube gradually diminifhes, conftantly keeping a certain proportion both with the diftance of the faid tube, and with the exceffive fire which continues to be exerted on the furface of the fame; therefore, the atmofphere of this tube will drive into thofe parts of the conductor which are remoter from the point A, a lefs quantity of fire than into thofe which lie nearer the fame point;

again,

again, therefore, the divergence of the electroſcopes muſt from A towards 2 B gradually leſſen, and at laſt vaniſh. III. The extremity of the atmoſphere of the glaſs tube, reaches (as we ſuppoſe) ſo far as 4 B; and ſince the fire which this atmoſphere endeavours in all this ſpace A 4 B, to drive into the conductor, cannot be condenſated within the ſubſtance of the ſame, it cannot of courſe enter it without driving an equal quantity of fire out of it, which will become manifeſted on ſome other part of the ſurface of the conductor, which the atmoſphere does not reach; for inſtance, in 4 B 8 B, or even in 3 B 4 B, whereto the atmoſphere indeed perhaps reaches, but only in a faint imperfect manner. IV. Hence two contrary forces will reſult, which will endeavour in contrary ways, to ſet the electricity in the conductor in motion; the one uneven and decreaſing, that is to ſay, that from the glaſs tube which, in proportion as the diſtance of it will be greater, will drive with the leſs force the fire lying on the ſurface of the conductor, into the ſubſtance of the ſame; and this fire, as has been obſerved, muſt unavoidably drive in its turn an equal quantity of interior fire from ſome other part of the ſame conductor. The other force will be uniform; it will be that of this fire thus forced out of the conductor, and kept out on its ſurface, which will *react* back, and endeavour to force anew out of the body of the conductor a new quantity of inward fire. V. The place where ſuch contrary forces will become equal to each other, will be (and let us call it ſo) the place of equilibrium: there no viſible electricity will obtain, the natural fire of the place being equally ſolicited there by two equal and contrary forces. VI. Let ſuch place be in the middle between 2 B and 3 B: from the latter point towards 4 B, the divergence by exceſs of the electroſcopes will begin, and keep gradually increaſing, according as the atmoſphere of the glaſs-tube gradually ceaſes to act upon the conductor: in the ſpace between 4 B and 8 B, to which the repreſſing atmoſphere (according to our ſuppoſition) cannot reach, the electroſcopes will diverge uniformly, in conſequence of the exceſſive fire which, as has been above obſerved, will be uniformly maintained on that part of the conductor. It indeed ſhould ſeem, that the electroſcopes ought to begin to diverge gradually the one more than the other, from the firſt

firſt point A, to the very place of *equilibrium* (which is the middle point between 2 B and 3 B), but really in the whole interval between 2 B and 3 B, no difference between the divergences of the electroſcopes can be obſerved; the oppoſition of the one force againſt the other, being in that whole ſpace too inconſiderable to produce any ſenſible decreaſe in the divergence of the electroſcopes. To the above obſervations we may add, that the equipoiſe between the whole uniform preſſure that takes place upon the ſpace 4 B 8 B, and the ſum of all the unequal preſſures effected by the different parts of the unequally efficient atmoſphere, on the ſpace A 4 B, may very well be compared to the equipoiſe by which water raiſed within a pail by ſtones, keeps its level, and balances itſelf with the numerous different preſſures effected within it, by the ſtones which are ſucceſſively made to raiſe it to different degrees of height. VII. That afterwards when the electrified tube is removed, all electricity vaniſhes (no part of the electricity from the glaſs, we have obſerved, could get into the conductor) is a plain and neceſſary circumſtance: the whole fire that was kept back from the part of the conductor immerſed in the atmoſphere, and had retired to the other part upon the ſurface of which it was kept, falls back into its natural place, as ſoon as the driving force is removed.

471. I now preſent the electrified tube perpendicularly under 4 B, that is, under the middle point of the whole conductor A 8 B; when I find that the divergence by deficiency of the electroſcopes does not extend farther than a foot and an half on each ſide of the point 4 B, that is, from 4 B to the middle point between 2 B and 3 B, on the one ſide, and to the middle point between 5 B and 6 B, on the other. II. I preſent the electrified tube under the extremity A of the ſecond conductor A 4 B, and the divergence by deficiency does not now extend farther than a foot and an half from A; that is, than the middle point between 1 B 2 B.

472. I. I now preſent the tube under the middle point 2 B of the ſame ſecond conductor A 4 B, when I find that the divergence by deficiency of the electroſcopes only extends to one foot; that is, to 1 B on the one ſide, and to 3 B on the other. II. I preſent the tube under the extremity A of the third conductor A 2 B,

and

and the divergence by deficiency only extends to one foot, from A to 1 B.

473. I prefent the tube under the middle point of the fame third conductor A 2 B; and the divergence by deficiency of the electrofcopes does not reach even to one half part of an interval. II. I prefent the fame under the extremity A of the fourth conductor A 1 B; and I likewife find that the divergence by deficiency does not extend even to an half part of the length of the conductor. But in all thefe different operations I find, that in order to produce the fame degree of divergence in the firft electrofcope A of each conductor, the tube muft be brought nearer to the conductor, according as the latter is fhorter.

474. The reafon of this is, that when the electrified tube is brought equally near to the extremities A of all the conductors indifcriminately, its atmofphere occupies a proportionally greater part of thofe conductors which are fhorter, and alfo occupies the fimilar parts of them with proportionally more efficient parts of itfelf. Therefore, in the fhorteft conductors, the electric atmofphere produces with its outmoft parts, a refiftance proportionably fuperior to the effect it endeavours to produce with its firft parts. With its firft parts it endeavours to drive into the fubftance of the conductor the natural fire which is diffufed over the furface of the fame near the extremity A; but then its actually producing fuch an effect, intirely depends on the poffibility there is, both that an equal quantity of natural fire may be driven from the infide of the conductor, and that it may accumulate on the furface of it near B. Now, as thofe fucceffive parts of the atmofphere of the glafs tube which extend to the extremity B, are the more efficient in proportion as the conductors are fhorter, they repel the more ftrongly the fire which the firft parts of this fame atmofphere endeavours to make fpring out of the fpot on which they act; and, therefore, oppofe the more the immediate effect which that firft part endeavours to produce in the vicinity of A.

475. Let, therefore, the tube be brought nearer to the fhorter conductors; it is true their extremity near B, will, by that means, be immerfed in a more efficient part of the atmofphere than before; but fince the efficiency of fuch atmofphere, undoubtedly increafes after

after a greater ratio than the fimple inverfe ratio of the diftance of the center of the faid atmofphere, the confequence of the tube being thus brought nearer, will be, that the efficiency of that part of the atmofphere which will furround the conductor near A, will have increafed in a much greater proportion than the efficiency of that part which will furround B, can have done; the effect of the bringing the tube nearer to the conductor, will therefore caufe the efficiency of the firft parts of the atmofphere to furmount the fooner the refiftance oppofed by the following parts of it; this efficiency will be able to make the electric fire fpring from fuch places of the fhorter conductors, for inftance, from B, as can yet give no fire, or very little of it, in longer conductors, or even ftill admit the fire which is driven into them. In confequence, therefore, of the tube being placed nearer the conductors, in proportion as they are fhorter, the place of *equilibrium* will draw proportionally nearer to A; and the two feries of contrary divergences, that of divergence by deficiency, and that of divergence by excefs, will converge the more rapidly, and with the more exactnefs, towards this point of equilibrium.

If it were poffible to bring the above experiments to a fufficient degree of accuracy, a vaft field would be opened here for Geometry to exercife itfelf. But I have proceeded as far, I think, as the fubject of this work can admit.

476. From what has been faid, every body furely underftands, that if the conductor be ftill more fhortened, and every way leffened, it will at laft be wholly furrounded by parts of an electric atmofphere, that will be very nearly equal to each other in point of efficiency; fo that as long as it will remain infulated, no fenfible excefs or deficiency will be manifefted around it. This is the cafe with a ftripe of gold-leaf infulated with a very dry thread of filk, and immerfed, for inftance, in the atmofphere of the Chain: the fire within it remains unmoved, and we may fay equally repelled from every fide; and it is only when I prefent my finger to it, that the tenfion of the exceffive fire of the atmofphere, drives the natural fire of the ftripe towards the finger, where fuch tenfion is weakened by it.

477. But

477. But if the body immerfed in the atmofphere be of any confiderable length and annexed to an infulating body, fo that this atmofphere may give fome fire to it, or draw fome fire from it, fuch body will move, be attracted, and then repelled, as often as it will be prefented to the electrified body that actuates this atmofphere. To explain this, I will alledge the example of the threads of the fcrutator E (Pl. VII. fig. 1.) I prefent them obliquely to a conductor (Y, Pl. I. fig. 1.) and they are attracted by it: this is becaufe the atmofphere of the conductor can then drive fome natural fire from the extremities of the threads which are prefented to it, towards the other extremities which are contiguous to the fealing-wax. I now prefent the fame threads to the conductor in a lateral fituation ftill, but fomewhat underneath, and they are repelled by it; the reafon of this repulfion is, that as the lump of fealing-wax has become, in the prefent fituation of the fcrutator, immerfed in the atmofphere of the conductor, fome fire is driven from it by that atmofphere, into thofe parts of the threads which are near the wax, and they become in confequence of this, electric by excefs.

478. Since I have mentioned here the fcrutator, I fhall by its means examine into fome other feeming irregularities (in reality there is no irregularity, their appearance can only proceed from our ignorance) that take place in fome extremely complicated motions of the threads of that inftrument, efpecially when I ufe it in order to try the electricity of the electric well. I. Let the well A (Pl. VII. fig. 5.) be electrified by excefs, and the fcrutator not be electrified at all. If I lower the latter along the axis or middle of the well, the two curves, expreffed in the figure, indicate the line traced by the ftripes of paper in their way down the well; that is to fay, when thofe ftripes begin to be immerfed in the atmofphere of the well, they begin with diverging, becaufe the atmofphere of the well then drives the natural fire of the threads up to the fealing-wax. But as the threads are immerfed lower and within a gradually more active part of the atmofphere, their divergence increafes; if they are lowered ftill more, they manifeft a kind of tendency to move upwards, as if drawn by the electricity of the upper part of the well; but fo foon as they are

carried to a certain degree of depth, they fall: the reafon of all thefe accidents is, that the atmofphere which is diffufed around the fides of the well, is now enabled by the fituation of the threads to drive downwards the excefs that has gathered near the fealing-wax; therefore, as foon as that excefs has returned to its former place, the threads fall, and continue to defcend fo far as to the bottom of the well, without any farther motion or divergence.

479. Let both the threads of the fcrutator and the well be electrified alike, for inftance, by excefs (Pl. VII. fig. 6.). Ufually in the act of immerfion into the atmofphere of the well, the threads are repelled and thrown back upwards, as being fimilarly electrified with the well; but when they come to be actually immerfed into the well itfelf, they are fomewhat attracted by its fides, becaufe the atmofphere of the well being very ftrong, introduces a change into the fituation of the fire in thofe threads, which it drives up to the wax. At a certain inconfiderable depth in the well, the excefs from the fides fo difpofes and equilibrates the excefs in the threads, that no divergence takes place. Below fuch place, the threads refume the fame divergence which they had in confequence of their own excefs before they entered either the well or its atmofphere, becaufe they then have reached a place where no atmofphere is exerted.

480. Let the threads of the fcrutator and the well be contrarily electrified. When the threads begin to be immerfed into the contrary atmofphere of the well, they diverge from each other, and manifeft a kind of tenfion; when they are immerfed into the well itfelf, they are laterally attracted by the fides of it, confequently diverge to a very great degree. Laftly, in the bottom of the well, where no atmofphere befides their own is exerted, they refume that degree of divergence which they derive from their own electricity.

481. But let us return to our experiments with the conductors mentioned above. Before I prefent the rubbed glafs under the extremity A of the conductor A 8 B, I take care to introduce a fmall excefs into the latter, and all the electrofcopes diverge accordingly. Approaching then the rubbed tube to this conductor, I ob-

I obſerve that the firſt electroſcope in A loſes its whole divergence by exceſs; then the ſecond alſo loſes its own; then the third, then the fourth, &c. ſo far for inſtance as the mark 1 B, to which the place of the equilibrium is brought in this experiment, whereas it formerly ſtood farther, between 2 B and 3 B. Then the firſt electroſcopes begin again to diverge, but by deficiency; and the divergence by exceſs of the remoteſt ones, at the ſame time increaſes.

482. If I experiment after the ſame manner with the other leſſer conductors *a* 4 B, *a* 2 B, *a* 1 B; that is, if I preſent under them the rubbed tube, after I have introduced into them ſome exceſs, the ſame effects reſult in them that I have above deſcribed; in all, the electroſcopes in the vicinity of *a*, at laſt manifeſt a divergence by deficiency; in all, the *place of equilibrium* is found to be nearer the extremity *a* than it was before; and, in all, the electricity by exceſs of the farthermoſt electroſcopes is increaſed; but with this circumſtance which muſt be taken notice of, viz. that when the conductors are ſhorter, the rubbed tube muſt, in order to produce the above effects, be brought nearer to *a*.

483. But if I make afterwards one of the above conductors communicate with the Chain, animated with a ſtrong electricity, then the glaſs tube can no longer deſtroy the divergence by exceſs of the neareſt electroſcopes, much leſs introduce at laſt into them an electricity by deficiency and then attract them; but the ſtrong active exceſs which is now exerted around thoſe electroſcopes, maintains its ground, and their threads continue to fly from the glaſs tube.

484. All the experiments above deſcribed muſt indeed appear ſurpriſing, if we conſider that they exhibit an inſtance of two contrary electricities combined together, as well as of a place where there is none, on one and the ſame body. But our ſurprize will increaſe ſtill more, if we carry farther our experiments on this ſubject, ſince we ſhall find, that we may both draw fire from one part of a body which has only its natural quantity, or even is deficient, and give this fire to the other part of the ſame body, which has more than its natural quantity. In fact, I. If while I hold the rubbed tube under one of the conductors, a perſon preſents his finger

either to the place where the electroſcopes diverge by exceſs, or to that where they do not diverge, or alſo to that where they diverge by deficiency, in all thoſe places he will draw off a ſpark. II. In conſequence of this drawing off a ſpark, the divergence by deficiency of the electroſcopes in the vicinity of A will increaſe, and be extended to ſome farther electroſcopes beyond them; but all divergence by exceſs in the farthermoſt electroſcopes will ceaſe. III. And if in ſuch ſtate of things I remove the tube, a ſmall divergence by deficiency immediately takes place in all the electroſcopes of the whole conductor.

485. The above accidents may be eaſily underſtood, if we conſider, that the exceſs which, in conſequence of the preſſure of the atmoſphere of the rubbed tube, is maintained on the ſurface of the conductor beyond the place of equilibrium, ſolicits every particle of fire within that part to ſpring out; and as the finger, wherever it be preſented, leſſens the reſiſtance from the ambient air, which keeps (as we ſhall ſee more explicitly in the following chapter) ſuch fire from actually ſpringing out, it follows, that a certain quantity of it muſt now be thrown from the conductor, &c.

486. If all the above experiments are repeated by preſenting a ſtick of ſealing-wax, or an oiled ſtick, to the conductor, inſtead of a glaſs tube, all the ſame phenomena will take place, but with contrary motions of the electric fire. I. The deficiency excited on the ſurface of ſuch ſticks will excite a deficient atmoſphere in the ambient air. Hence the natural fire diffuſed in it will grow relaxed around the ſurface of the ſticks; it will raiſe an exceſſive electricity on the neareſt part of the conductor to which it is preſented, and cauſe the fire diffuſed on the farthermoſt ſurface of it, to flow back, and come to accumulate itſelf on the ſaid neareſt part. II. Thence will reſult on this neareſt part a divergence by exceſs; in the farthermoſt part, a divergence by deficiency; and, in its proper place, an equilibrium. III. The ſtick being kept under the extremity A of the conductor, and a finger being preſented to any part of it, a ſpark will be thrown into the conductor. The cauſe of this is, firſt, that every particle of natural fire within the conductor, is ſolicited to move towards the

the farthermoſt part of it, in order to fill up its deficiency. The property of all deferent bodies, of conſtantly preſerving the ſame quantity of fire, prevents any fire from ruſhing into the conductor; but when a ſtrange body is preſented to this conductor, as much fire runs to it, as can be admitted; and this fire ſupplies the deficiency at the other end of the conductor. IV. Therefore, the divergences on the farthermoſt part of the conductor, will be ſuppreſſed in the ſame manner as thoſe by exceſs were, in the former experiment; the contrary divergences on the neareſt parts of the conductor will increaſe alſo in a like manner, in a like manner the *place of equilibrium* will change, &c.

487. We may obſerve, that theſe experiments made on *unmoved* electric atmoſpheres, applied or preſented to deferent bodies, may be varied *ad infinitum.* I. By varying the ſhape and the capacity of theſe bodies; to which they are applied. II. By varying the nature of the *unmoved* atmoſphere, ſince it may be actuated either by an *exceſſive*, or a *deficient* electricity. III. By varying the ſtate of [the ſame deferent bodies; thus they may be electrified before hand, either by exceſs, or by deficiency, more or leſs ſtrongly. Again, if they have no electricity, they may or may not be inſulated. IV. By varying the diſtance at which the unmoveable atmoſphere is preſented to them. By applying either jointly or ſucceſſively, two or more ſuch *unmoved* atmoſpheres, whether homologous or contrary, to divers places of thoſe bodies. But every body may ſee, that to dwell on all theſe different complicated caſes would be infinite, and even ſuperfluous, ſince the ſimple ones above explained may afford a ſolution for them all.

488. Nay, from the above facts and reaſonings, we may foreſee what the caſe would be, if two atmoſpheres both *moveable*, were made to unite together. I take a tube made of tin, inſulated on a long handle of ſealing-wax (Pl. V. fig. 1.) equal in every reſpect to the part A 3 B (Pl. VIII. fig. 8.) of the whole conductor A 8 B, let us therefore call it *a* 3 *b*. I electrify it by exceſs with the hook of a bottle, and place it near the part A 3 B of the conductor A 8 B, ſo that their ſides are parallel, and only three inches diſtant from each other. I. The electroſcopes in A 3 B, along the ſide of *a* 3 *b*, manifeſt a divergence by deficiency. II. This divergence by deficiency ſoon leſſens, and then vaniſhes

in

in all that part of the conductor A 8 B, which lies beyond A 3 B; that is, beyond that part to which *a* 3 *b* correfponds. III. Then the electrofcopes in 4 B begin to fhew a divergence by excefs; and the following ones in 5 B, 6 B, 7 B, 8 B, likewife manifeft an exceffive electricity, which is fomewhat greater than that manifefted by the former, and befides is uniform throughout.

489. Thefe phenomena are of the fame kind as thofe which take place when the *unmoveable* atmofphere of the rubbed glafs tube is brought under the extremity A of the conductor A 8 B, and have a fimilar caufe. I. The moveable exceffive atmofphere of the conductor *a* 3 B, drives the natural fire from the furface of the other conductor A 8 B into the fubftance of the fame, and forces an equal quantity of fire to iffue and keep out on its furface, in the part 4 B 8 B. II. The action of fuch excefs againft the preffure of the atmofphere, produces, in a certain place fomewhere beyond 4 B 8 B, a place of *equilibrium*, &c.

490. There are, however, a few differences between the latter experiment and the former ones, which muft be taken notice of. I. The *moveable* atmofphere of the conductor *a* 3 *b*, remains applied to A 3 B, by parts of itfelf which are equally diftant from the fame *a* 3 B, that is to fay from the body by which it is actuated, and which are therefore equally efficient throughout, fo that it feems that an uniform deficiency ought to take place all over A 3 B; but as the feveral parts of the atmofphere of *a* 3 B, co-operate and act with more force about the middle of it than at either of its extremities, they create both a greater divergence, and a greater attraction in the electrofcopes fituated in that part, than in thofe at the extremities. II. The moveable atmofphere of the conductor *a* 3 *b*, ought, it feems, in confequence of its parallel pofition, exert a very great efficiency in driving the natural fire from the correfpondent furface of A 3 B; but yet, as it of itfelf poffeffes much lefs activity than the rubbed glafs tube, it cannot reach to fo great a diftance as the latter does; hence the place of the equilibrium ftands no farther than between 3 B and 4 B, very near the extremity 3 *b* of the conductor *a* 3 *b*; the divergences by excefs from 4 B to 8 B, are alfo lefs confiderable, &c.

491. If I previoufly introduce a certain degree of excefs into A 8 B, I find, that the divergences on its part A 3 B, are lefs, when

when the conductor *a* 3 *b* comes to be presented to it, than they were before; the *place of the equilibrium* lies still nearer to *a* 3 B, and the divergences by excess in 4 B 8 B are greater than formerly. The reason is, that the excess previously introduced into the whole conductor A 8 B, prevents so great a deficiency as that before from taking place in the part of it A 3 B; and, in the meanwhile, as the sum of the excess in 4 B, now consists both of the excess that has been introduced into it at first, and of that which has been driven into it from A 3 B, it, of course, proves greater in this experiment than it did in the former.

492. If the conductor *a* 3 *b* be placed near the conductor A 3 B, when a deficiency has been introduced into the latter, the divergences by deficiency will become greater; the deviation of the electroscopes towards the conductor *a* 3 *b* will likewise be greater; the place of the equilibrium will be removed farther off beyond 3 B, and the divergences by excess in 4 B 8 B will be proportionally less than before, or even there may be none.

493. If in each of these cases the conductor *a* 3 *b* be moved along A 3 B, keeping all the while its parallel position with respect to it, the divergences by deficiency will follow the conductor *a* 3 *b* in its progress, and at each end of it there will now be a distinct place of *equilibrium*, which will be accompanied by a place of divergence by excess.

494. If, in each of the above experiments, the electricity of the conductor *a* 3 *b*, or also that in the conductor A 3 B, be inverted, the same phenomena will take place, but after a contrary manner, conformably to what has been explained above.

495. Universally; since, when an atmosphere of a given kind is presented to a body animated by an electricity of the same kind, the electroscopes annexed to that body both lose their divergence, and tend to acquire a contrary one, so, when an atmosphere of a given kind is presented to a body impregnated with a contrary electricity to its own, the electroscopes of the latter tend to increase their former divergence, or tension; because, in this case, the atmosphere of the body we suppose, endeavours to increase that contrary electricity with which it now meets.

496.

496. I ſay divergence, or tenſion: in order to explain this, I muſt remind the reader of this principle, that *bodies ſimilarly electrified repel each other, and bodies contrarily electrified attract each other.* Therefore, when I preſent the rubbed tube to the electroſcope A of the conductor A 8 B, which is electrified in the ſame manner with it, the firſt motion of this electroſcope is always to fly back; though, it is to be obſerved, ſuch motion is ſhorter in proportion as the electricity previouſly introduced into A 8 B is leſs, and as the capacity of this ſame A 8 B, beyond the limits of the atmoſphere of the rubbed tube is greater; the reaſon is, that in the latter caſe, the electricity in A, of the conductor A 8 B, which occaſions the above motion, is the ſooner diſſipated by the atmoſphere of the glaſs tube, and the ſooner alſo turned into a deficiency; ſo that the electroſcope may be already attracted, nay, may be already electrified by deficiency, though it has not had ſufficient time to appear ſenſibly repelled, and to fall gradually when loſing its divergence by exceſs: in order, therefore, to be able to diſcern the inſtant of ſuch a material change as that which takes place in this electroſcope, the rubbed tube muſt be preſented to the conductor A 8 B, from a conſiderable diſtance, and very ſlowly. Again, if the weather be very favourable and dry, and if the electroſcopes are made with very dry threads, a new accident will take place, of which I muſt inform the reader, which is that the cork-balls will at firſt fly to the conductor; the reaſon is, becauſe the rubbed tube will have introduced a deficiency into the very *deferent* conductor, before the dry threads can have had time to loſe their own exceſs; whence the cork-balls, being ſtill electrified by exceſs, will at firſt run to the conductor, which has already become ſo by deficiency.

497. Now, with regard to the tenſion of the threads, which I began to mention, I obſerve, that as ſoon as this tenſion takes place, it is a ſure ſign of their electricity being turned into a contrary one. As long as there remains in the electroſcopes any electricity homologous to that which produces the atmoſphere around them, the ſaid electroſcopes proportionally fly back (they always fly

fly back when their own homologous electricity cannot be driven away, either on account of its intenſity, or for want of a capacity into which it may loſe itſelf) and alſo proportionally diverge. Afterwards, in the inſtant when there remains no electricity whatever in the electroſcopes, the threads of the ſame naturally hang ſomewhat ſhrunk in and contracted, as hanging threads generally do. But, laſtly, when the ſurrounding atmoſphere now begins to introduce into the electroſcopes an electricity contrary to that which they had at firſt, then the threads begin to be proportionally directed to the body, from which this atmoſphere ſprings; and they do this with ſuch a degree of tenſion, as makes them loſe their tortuous appearance. Whence it happens, that they indeed both diverge from each other, in conſequence of their being again animated by the ſame kind of electricity; but, it is to be obſerved, that this divergence is leſſened by their common tendency towards the rubbed glaſs, which now attracts them; ſo that the whole intenſity of their electricity is not to be eſtimated from the degree of their divergence only, but in order to value it with accuracy, account muſt alſo be had of the ſaid tendency.

498. Every one who knows how important it is in experiments on *natural,* and much more *on artificial,* Electricity, accurately to diſtinguiſh the different changes of an electricity into a contrary one, will not, I am certain, think that I dwell here upon too minute objects. However, I now proceed to deduce, from what has been ſaid on the reciprocal actions of electric atmoſpheres, a more exact determination of a principle, which is of the greateſt importance in Electricity; I call it *the principle of, both the equilibrium, and the motion, of the electric fire.*

CHAP. IV.

On the duration of electric atmospheres, and on the state of electricity in air which is continually renewed. More complete principles with respect to the equilibrium and the motion of the fire. On the general motion of the electric fire contained in a body, towards that place whence a spark is drawn; and on a certain curvature which takes place in this spark, in consequence of this same motion, the law of which is ascertained.

499. DR. Franklin has often found, that after turning round over his head, more than one hundred times, an electrified cork-ball, the same still possessed a degree of electricity. Mr. Kinnersley (Franklin, p. 384.) communicated some electricity to another person, by throwing him his hat. I have often tried to electrify with a glass tube those long iron wires with which I use to examine the electrical state of the atmosphere, when the state of the latter was such, that they derived of themselves very little, if any, electricity from it; and always found, that in serene and windy days, they preserved their electricity as long as in serene and calm weather.

500. Now, this may appear a kind of paradox, that in air, which perhaps is renewed a thousand times, the same attractions and repulsions should continue to take place, as do in unchanged air; but the nature of electric atmospheres, which we have before explained, likewise explains the wonder of this phenomenon. The electricity of a body resides within the superficial pores of it, and actuates the ambient air, not by diffusing itself into it, but by exciting either a tension or a relaxation in the natural fire inherent in it; therefore, that air, as soon as it is left (through the progressive motion of the electrified body) behind the limit to which the action of the latter extends, recovers its natural state: the air which takes its place, in its turn experiences a tension (or a relaxation) in its own natural fire, in the same manner as the former did; and thus both *the actuating electricity*

city in the body, and the actuated atmofphere in the ambient air, continue to remain the fame, in the fame manner precifely as would happen, if the fame identical air continued around the electrified body.

501. But this propofition with regard to the duration both of electricities, and electric atmofpheres, in renewed air, will be rendered more fimple, by confining it to its true limitations. The firft exception to the above propofition is, that when the electricity raifed in a body is extremely ftrong and intenfe, then indeed this electricity, as well as the atmofphere of that body, do not laft fo long in continually changed, as they would in calm, permanent, air. I charge two equal bottles at once, till the charge gufhes out of the hook of both, then I make one of them whirl round over my head with great rapidity; and, I find, that the ftrong exuberant excefs in it ceafes fomewhat fooner than in the other bottle which I left unmoved. The reafon is, that, as the excefs which diffipates from the unmoved bottle continues to meet with the fame permanent air, it cannot but be in fome degree retarded; whereas the excefs which diffipates from the whirled bottle, fprings out of it fomewhat more readily, as it continually meets with air free from any excefs. The other exception is, that if the ambient air is not of itfelf infulated with a fufficient degree of exactnefs, the mixture of a new quantity of it, will leffen the electricity of the bodies within it. Thus, if I open both the window and door of my room, when the weather is damp, the electricity of my apparatus, however weak it may already have grown, will abate ftill more in confequence of this introduction of exterior and new air into the room; the additional vapour brought in, in the latter cafe, muft neceffarily carry away a quantity of electricity, befides that which has been before diffipated.

502. However, we may here obferve, that the continuation of the electricity of a body in renewed air, (under the above limitations) though it has hitherto been one among the innumerable myfteries of nature, is now demonftrated to be no more than a confequence of what has been above explained, both concerning *actuating* electricities, and the manner in which they operate on the atmofpheres of bodies.

 503. Avail-

503. Availing myfelf of the above knowledge, I fhall now proceed to lay down *a principle refpecting both the equilibrium and the motion of the electric fire*, more comprehenfive than that which I ufed in former places to propofe. Hitherto I have ufed to take notice only of the expanfive force of that electric fire which lies diffufed on the furface of bodies, but now I propofe alfo to take into confideration the action which refults from the tenfion of the electric fire in the air. In fact, *fuch tenfion (or action) of the electric fire inherent in the air, is, of itfelf, fufficient to alter the equilibrium of the fire in the bodies within it, and always contributes either to form, or to reftore, this equilibrium.*

504. When I prefent a rubbed glafs tube to the head A (Pl. VIII. fig. 8.) of the brafs conductor A 8 B, it is only the exceffive tenfion of the electric fire inherent in the air that furrounds the rubbed tube, that drives the natural fire from the extremity A of the conductor, towards the extremity B. When I prefent to the fame conductor a rubbed ftick of fealing-wax, then it is the relaxation of the fire inherent in the ambient air, that makes the natural fire in the conductor flow back from B towards A; therefore, a fimple alteration in the tenfion of the electric fire in the air (whether the fame be increafed or leffened), will alter the equilibrium of the fire inherent in thofe bodies which are immerfed in it.

505. And this fame tenfion, as long as it remains in its natural degree, contributes to produce the fame equilibrium of the electric fluid; becaufe, fince the alteration of this tenfion alters the balance of the electric fluid, and the alteration of this balance reciprocally alters fuch tenfion, it follows that the equilibrium that takes place in the fire proper to bodies, whenever it obtains, muft be in a great meafure preferved by the fire inherent in the ambient air around them, when the latter likewife poffeffes its natural degree of tenfion.

506. That, afterwards, when the natural equilibrium of the fire in bodies happens to be altered, it may be reftored by the tenfion of the fire in the air, the degree of which is likewife altered, is what manifeftly refults from every experiment. When I touch an electrified body, I take from the fame both its actuating elec-

tricity

tricity and its atmofphere; therefore, the pofitive or negative force of fuch atmofphere has been employed, either in expelling the exceffive fire, or in fupplying the deficient one; that is to fay, the exceffive tenfion of the fire proper to the air around that body, has been employed in driving off the fire exuberating in it; or in the cafe of a negative electricity, the relaxation, or deficient tenfion, of the fire in the air around the deficient body, has facilitated the introduction of new fire into it.

507. But here is another experiment, with which I ufually alfo prove the above mentioned functions of electric atmofpheres. With a bit of wax I faften a needle on the tube A 3 B (near 2 B) perpendicularly to it; then I move the glafs tube, ftrongly electrified, backwards and forwards in the vicinity of the needle; this done, I carry the rubbed tube near the conductor, and parallel to it; when a fpark uncommonly ftrong is thrown out. If the operation be made by two perfons, and two rubbed tubes be thus prefented to the conductor, the fpark will be ftill ftronger, and with regard to its crack and light much refemble that of the difcharge of a bottle.

508. Now, if *ftrange* atmofpheres can thus produce fuch uncommon fparks, furely the atmofphere itfelf proper to a body, muft alfo contribute to excite fparks from it. The concurrence of the atmofpheres of the above rubbed tubes, increafes the tenfion of the air around the conductor to fuch a degree, that befides the exuberant fire, it moreover drives the natural fire out of it; and in fact, when, after exciting the fpark, the tubes are removed, the conductor is found electric by deficiency. If, inftead of tubes of glafs, fticks of fealing-wax are ufed, the re-union of their atmofpheres fo much increafes the relaxation of the fire in the air around the conductor; that, befides the deficient fire, even exceffive one runs to its furface, fo that, the fticks being removed, the tube is found electric by deficiency. Therefore, we are well grounded to fay, that the tenfion alone which arifes in the fire proper to the air around a body, by virtue of the fire which exuberates in that body, contributes to repel and reprefs this fame exuberant fire: and the relaxation alone, which takes place in the fire proper to the air around a body, by virtue of the deficiency

which

which takes place in it, contributes to determine towards it a proportional quantity of fire to fupply that deficiency.

509. Nor is there any danger that atmofpheres proper to bodies will not operate in the fame manner as *ftrange* ones, fince the tenfion of the fire proper to the air around a body, operates from a diftant circumference towards the furface of that body, with a force equal to that with which it would operate from the furface itfelf of the body, towards the faid circumference, conformably to the univerfal law of equality between actions and re-actions. The above manifeftly agrees with experiments; when I prefent the rubbed glafs, or fealing-wax, under the extremity A (Pl. VIII. fig. 8.) of the conductor A 8 B, then the atmofphere which takes place near the extremity 8 B, is balanced by that which furrounds the extremity A; therefore, the latter atmofphere operates againft the conductor, in a direction that is from the furface of the rubbed glafs, or wax, towards its own circumference; and the atmofphere proper to the conductor balances the other atmofphere, by operating in a contrary direction to it; that is, from its own circumference, towards the furface of the conductor.

510. Having thus demonftrated this truth, that the force of the fire proper to the air around bodies, jointly concurs with the force of the fire proper to thofe bodies themfelves, in creating the motions of the electric fluid, it now remains that we fhould bring this truth to its proper univerfality; becaufe, it is not only in the electricity of a fimple excefs, or a fimple deficiency, that the abfolute tenfion or relaxation of the fire in the ambient air is to be confidered, but in all kinds of electricity, univerfally, (93) the overplus of tenfion in the air around one body above the tenfion in the air around another body, will contribute to the greater efficiency of the former above the latter's, in determining the motion of the electric fire; fo that, I. If we treat of an electricity of an abfolute excefs, fuch, for inftance, as that in the Chain with regard to the ground, then we may fay, the fire that exuberates in the Chain, is determined to pafs into the grounds in confequence of the fuperiority of its expanfive force above that of the fire in the ground, and alfo by the fuperio-

rity

rity of the tenfion in the air around it, above that which obtains in the air contiguous to the ground. II. If we treat of an electricity of an abfolute deficiency, fuch as that, for inftance, which takes place in the Machine relatively to the ground, then we ought to fay, the natural fire in the ground will be determined to flow into the Machine, in confequence of the fuperiority of its expanfive force above that of the defective fire in the Machine, and alfo by the fuperiority of the tenfion in the air contiguous to it, above that in the air contiguous to the deficient Machine. III. If we treat of a *refpective* electricity by either excefs, or deficiency, then we are to give the fame explanations with regard to the particular bodies in which fuch electricities take place, as have been given in the cafes of the Chain relatively to the ground, or of the ground relatively to the Machine. IV. If we treat of a *refpective* electricity *by excefs and deficiency together*, then we ought to fay, the fuperiority in point of expanfive force, and of tenfion, that takes place, in and around, the one of the two bodies, will increafe in confequence of both the excefs of fire in that body, and the deficiency of it in the other. V. If we treat of *refpective* electricities either *of unequal excefs*, or of unequal *deficiency*, then we are to fay, the force which produces the motion of the electric fire will leffen, becaufe the fuperiority of expanfive force, and tenfion, in and around the one of the bodies, will leffen proportionably to the expanfive force and tenfion in and around the other body.

511. Thus it will be univerfally underftood, how the motion of the electric fire between two fyftems, or bodies whatever, is determined, both by the overplus of the expanfive force in the fire on the furface of the one of them, above that of the fire on the furface of the other, and by the fuperiority of the tenfion in the air around the former, above the tenfion in the air around the latter.

512. The *refpective* excefs of the tenfion in the air around a body A, alfo contributes to the motion of the electric fire which, in any degree, fuperabounds in that body, by determining it to flow towards that part of it, where this tenfion becomes weakened, in confequence of the approach of another body B, which is, in any degree defective with regard to the body A. Thus, when

when I prefent my finger to the Chain, the exceffive fire of the Chain drives off the natural fire from the furface of my finger, and thus introduces a kind of very fmall charge over it: now, this driven fire is like a fupport that is taken away from both the exceffive fire in the Chain, and that in the intervening air; therefore, the exceffive fire around the whole Chain, drives a fomewhat greater excefs towards the place where I approach my finger, then towards any other part of the Chain. When I prefent my finger to the Machine, then, I fay, the natural fire that is *wanting* on its furface, is like a fupport taken away from the natural fire of the intervening air; therefore, the natural tenfion of the electric fire in the air that furrounds my body, drives the natural fire from the whole furface of it, and makes it flow towards my finger; then this fire, in its turn, drives a new portion of fire from the part of the Machine which lies neareft to it, and thus increafes the deficiency in this fame part; which muft proportionally fill up and leffen the fame in all the others.

513. With regard to this *flowing* of the fire of an electrified fyftem, towards that part of it to which another fyftem, or body, is brought near, I fhould perhaps fear left I have been fomewhat too diffufe, if I were not convinced of the importance of the fubject, and of the great light which the experiments I have related procure to the fcience of Electricity. I even fhall add another, which is fimple, and, in my opinion, decifive. When your electrofcope of the Chain, diverges a little, in confequence of a very fmall degree of electricity being diffufed into the latter, approach flowly to it, the back of your hand, and you will fee the balls of the electrofcope, which were hardly feparated, recede from each other more than to three lines diftance; efpecially if your conductor be very large, fuch as that in fig. 1. Pl. I: in order to produce the above fmall electricity, you only need approach the rubbed glafs tube to the conductor, and by keeping it at a certain diftance from the conductor, you will be able to raife as fmall an electricity as you pleafe.

514. The above is to be explained in the following manner: the tenfion of the fire in the air around the conductor, where it has its natural fupport, makes the fmall excefs in the conductor, run

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to the place to which the hand ſtands near, and deprives the fire in the air of its ſupport, in conſequence of the free paſſage that obtains through the hand into the ground. If the conductor be electrified by deficiency, in this caſe a certain quantity of exceſſive fire will run to the above part of the hand, and to the air contiguous to it, in conſequence of the tenſion in this air being now increaſed; which exceſſive fire will, in its turn, drive a farther portion of the natural fire remaining in the conductor to another part of it, when the tenſion of the natural fire in the air around it, is deficient and below the natural degree.

515. Now this *flowing*, juſt now deſcribed, of the exceſſive fire of a body to ſome one part of the ſame, under certain circumſtances, this *flowing*, I ſay, when it is effected with a certain degree of force, cauſes this fire, in that place whereto it is determined by the approach of a ſtrange body, to divide the intervening air, in the very ſame manner as a ſtrong ſpark ſometimes breaks through a ſheet of glaſs that oppoſes its free paſſage. In regard to this fact, I have, beſides, another experiment which confirms both the reality of the above *flowing* of the exceſſive fire, and its power of determining, according to a certain ſettled law, the direction of the ſpark that is thrown out in conſequence of it, which law, as far as I know, has not been mentioned by other Writers.

516. I take a braſs rod A B (Pl. VIII. fig. 9.), which is ſolid and half an inch thick, and terminated in A, into an hemiſphere; I preſent it to the conductor Y (Pl. I. fig. 1.) animated by a ſtrong electricity, and bring it near the ſame in a ſituation almoſt parallel to it, in ſuch a manner that its rounded end be the neareſt to the conductor; when it conſtantly happens that the ſpark flies to the head of the braſs rod in an incurvated direction, the cavity of which is conſtantly turned towards that part of the conductor along which the rod is placed. Likewiſe, when I preſent my finger to the conductor, after the ſame manner as I did the braſs rod A B, the ſame effect ſtill obtains; that is, the ſpark flies to the tip of my finger in an incurvated direction, the cavity of which is turned towards my finger. In ſhort, the perſon who experiments has it in his power to make the ſpark bend its way in any given direction;

 only,

only, it is neceffary to excite the fpark from fuch parts of the conductor as are free from afperities.

417. The above effect is to be explained thus; the *wave* of exceffive fire from which the fpark refults, is determined by the fuperior tenfion of the fire in the air around the conductor, to flow to D (a place where the ftratum of the intervening air is leffened, and confequently refifts lefs) along the fide CD of the conductor, collaterally to which the tenfion of the fire in the air grows continually lefs and lefs fupported, in confequence of the fire which this *wave* becomes enabled to drive into the ground, both from the other furface of the fame air, and from the brafs rod. Therefore, if this direction of the wave is only confidered, we fhall find, that it muft proceed directly towards E; but with fuch direction we are to compound the force which will refult from the refiftance of the ftratum of air through which the fpark is to break; and this refiftance, by gradually turning the fpark out of its way, will make it bend its courfe towards the end A of the brafs rod.

518. But here I find that the *preffing* electricity of electric atmofpheres, has led us, in a manner unawares, to confider the vivid electricity of fparks; a fubject which is as extenfive as it is important, and muft be treated in a fection apart.

SECTION

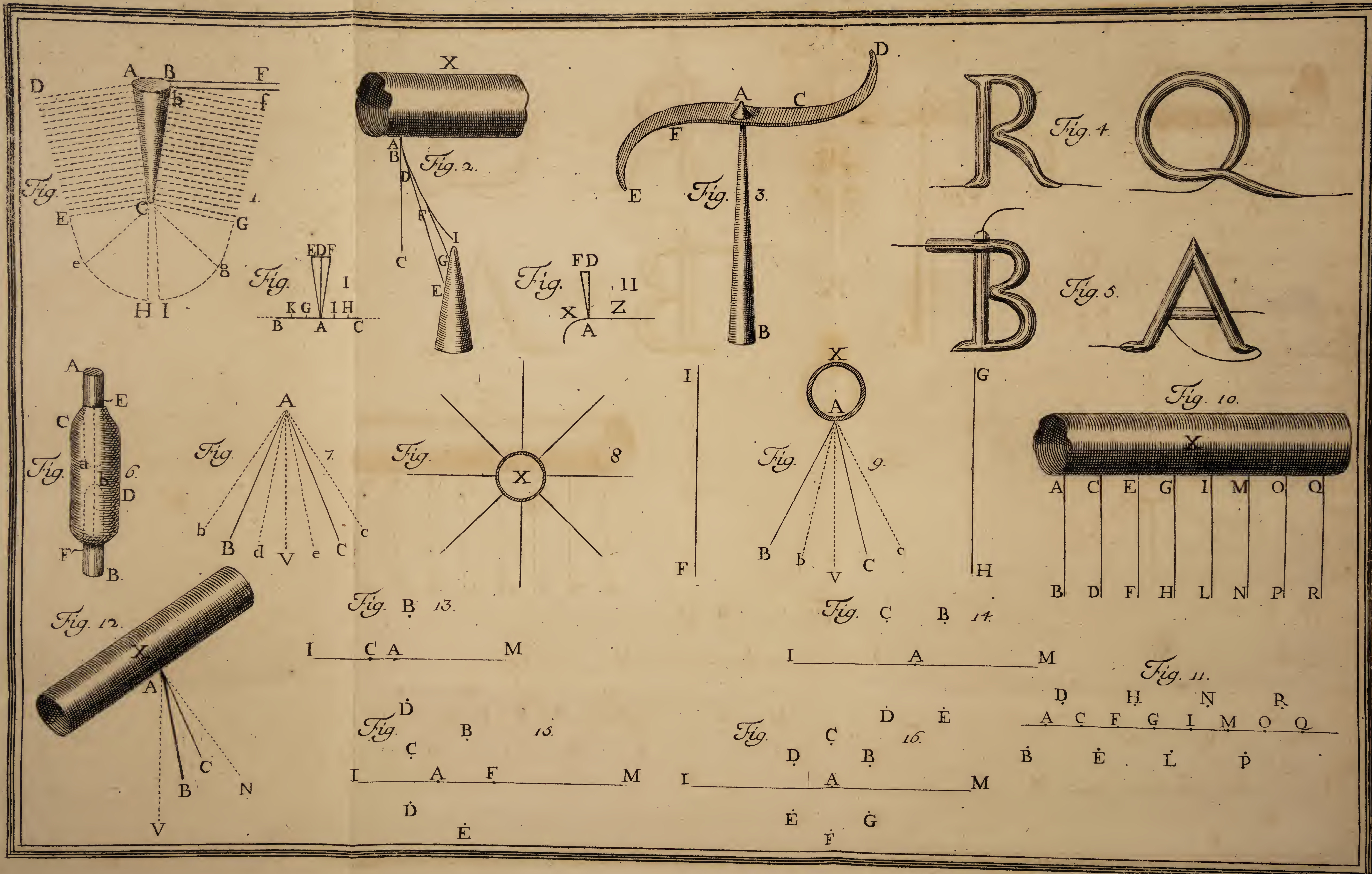

Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8
Fig. 9.
Fig. 10.
Fig. 11
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.

IV

SECTION ~~III~~.

On vivid Electricity; otherwife, on electric Sparks.

CHAP. I.

On ſparks relatively to the air.

519. WE have hitherto conſidered the modification of the preſſure that takes place in the electric fire proper to the air around bodies, in conſequence of the electric fire that is added to, or taken from the ſurface of thoſe bodies; now we are to conſider the action of the air againſt the fire, exuberant relatively to it, that paſſes through it under the ſhape of a *ſpark*, and alſo that of the ſpark againſt this air. In regard to theſe reciprocal actions, two things muſt be attended to; the one is, the kind of proportion according to which the air reſiſts the ſpark; the other is, the peculiar kind of action which electric fire exerciſes on air. To that end, we muſt chiefly obſerve three accidents; I. The *extenuated* ſhape of the ſparks. II. Their crooked dirction. III. Their diviſion.

Sect. I. *On the reſiſtance which air oppoſes to ſparks.*

520. *Air reſiſts ſparks in proportion to its own denſity; in proportion to the thickneſs of the ſtratum it oppoſes to them; and in proportion to the length of the paſſage which ſparks are led to open to themſelves through its ſubſtance.* In order to conclude *univerſally*, that air reſiſts the electric fire, it is enough to obſerve, that it forms, jointly with other inſulating bodies, the general *incloſure* which preſerves on the ſurface of bodies their electricity. In regard to the *particular* concluſion above expreſſed, viz. that air reſiſts the electric fire, according to the above mentioned proportions, the experiment I formerly deſcribed in num. 114 of *Art. Elec.* will be of great ſervice to prove it. With wax made ſoft with turpentine, I fix a re-

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ceiver on the bafon of the air-pump, with a brafs rod adapted to it, in fuch a manner that this rod may be raifed or lowered within it at pleafure (Pl. IV. fig. 12, 13, 14, 15.) I make this rod communicate with the Chain, and before I draw off the air, I take notice, that, from the extremity B of the rod (fig. 12.) which remains at fix inches diftance from the bafon, the brufh fprings downwards from the rod, in the ufual manner, to the diftance of about an inch. This done, I make the air to be fucceffively drawn from the glafs-bell; and, at every ftroke of the embolus, I obferve that gradual changes fucceffively take place in the brufh, and that in proportion as the air is farther extracted: I. The rays of the brufh are lengthened. II. Their mutual divergence is leffened, III. as well as their number, IV. and their fparkling. V. The width of the remaining rays increafes, VI. as well as their continuation; fo that, when the mercury of the barometer within the glafs-bell has been brought to be no more than an inch high, there remains only four or five rays, which, from the rod, reach quite down to the bafon; and if the mercury be ftill more lowered, till it is brought to be only four lines high, there remains nothing under the bell (the brafs rod muft then be a little lowered, as in the fig. 14.) but a continued red-violaceous ray. To all thefe obfervations the following fact is to be added, viz. that in proportion as the air grows rarified, at leaft beyond a certain meafure, the electric figns leffen in the Chain; fo that when the air is brought to its greateft rarity (this I obferved before with refpect to the experiment of Dr. Hauksbee), fparks intirely ceafe, and the electrofcope diverges no longer.

521. If the rod A B is left at a great diftance from the bafon, and efpecially if it be very fharp, then, if the air be extremely dilated, the fingle continued ray ceafes to appear, but a fhort light appears on the faid point, like the light mentioned in numb. 139, 141. and has been called light of *diffufion*, which extends to a certain diftance in the direction of the fides of the point, and grows gradually fainter, fo that at the diftance of an inch it is perceived no more. But when the experiment is made in the dark, a very weak light is difcerned, which extends from the point, and forms

forms an ample, though very rare, brufh, like that reprefented in fig. 15; and a very good eye may, moreover, if the darknefs be complete, perceive a kind of fpurious brufh (119) more languid and fhort, which reaches the knob C on the bafon of the air-pump. When the rod is thus placed at a great diftance from the bafon of the pump, whatever may be the rarity of the air, a faint motion continues to be difcernible in the electrofcope annexed to the Chain; this is, becaufe the air, notwithftanding its great rarity, yet refifts in proportion to the great height or thicknefs of its ftratum (the rod is here fuppofed to be ten inches, or a foot diftant from the bafon); it, therefore, keeps back a part of the electric fire, which would, if the ftratum of air was lefs, intirely and freely pafs through it. For the like reafon alfo, the exceffive fire which through a fhort ftratum of rarefied air, readily flows, united into a continued ray, divides and fpreads itfelf, when the fame ftratum is lengthened into thofe long and almoft invifible flafhes.

522. But, in order more accurately to diftinguifh the changes with refpect to its form and continuation, which the electric fire fuffers in air which is gradually more and more dilated, the experiment muft be made with rods armed at their ends with balls (Pl. IV. fig. 6.). Let fuch rods be brought to the diftance of one inch from each other; then, when the vacuum is rendered as exact as poffible, a fingle, continued, uniform ray, of a red-violaceous colour, is feen in the dark to flow from the rod B to the rod C. In proportion as air is afterwards admitted, this ray acquires, firft a kind of quivering motion, which indicates the beginning of an interruption; then, a real interruption with an undulation and divifion of the ray, take place (efpecially if the balls be a little removed from each other); afterwards the ray acquires a fomewhat more vivid light, fo that, through a continued fucceffion of changes, this quiet, continuous ray, at laft turns into very vivacious fparks, difcernible even by day-light, though, it is neceffary to obferve, they are emitted only with fuch interruptions as are neceffary to make the fire rife in the Chain to a fufficient denfity, to enable it to furmount at once the refiftance oppofed by the external air.

523. If

523. If the balls B, C, are kept at a ſtill greater diſtance from each other, then the ray that joins both begins to appear leſs uniform, though the air be dilated to the utmoſt, and an interruption of the fire takes place in the middle between the two balls, which increaſes as they are removed farther from each other. About twenty years ago I had a thought of amuſing myſelf with a kind of electric lucid writing; I procured glaſs tubes bent into the ſhape of letters, like thoſe repreſented in the Pl. IX. fig. 4, 5; iron wires were hermetically inſerted into their extremities, and after the air had been accurately drawn out of them, they were hermetically cloſed. But I had ſcarcely begun to experiment, when I perceived that a defective reaſoning had led me into a vain attempt: ſo far was I from illuminating the whole ſeries of letters (which communicated together through the iron wires), that I could not completely illuminate even a ſingle one of them, with the whole diſcharge of a bottle; the reaſon was, becauſe the ſpace was too great to allow the ſpark, however vehement, to go through the dilated air contained in the above tubes.

524. In the barometric vacuum, as it is more complete, the light of a ſpark reaches to a much greater diſtance. While I ſtand inſulated (as I ſaid in pag. 48 of the *Terres Atmoſp. Elect.*) and communicate with the Chain, I touch the hollow ball into which the upper part of my barometer is terminated, and a little ſpark leaps between my finger and the ſaid ball; meanwhile, an electric light, of a purpureous colour, fills the whole empty ſpace in the barometer, ſo far as the ſurface of the mercury; but, on the other hand, it is to be obſerved, that this light is more dilated, and affects leſs the eye, in proportion as it ſurpaſſes in point of extent the ſpark that has been thrown from my finger to the ball.

525. Theſe experiments may be varied at pleaſure, and even are rendered more ſignificative by cauſing the ſparks to paſs intirely through a barometric vacuum; which may be effected with the double barometer ABCDE (Pl. VIII. fig. 10.) contrived by the ingenious philoſopher, Lord Cavendiſh. I have ſeveral barometers of this conſtruction, in which the empty ſpace BCD extends to a diſtance ſomewhat greater than a foot. From the bottom of the two ſmall canns A, E, into which the two arms of the

the barometer are inferted, two iron wires rife, terminated into balls; I grafp the one of thefe, and prefent the other to the Chain; then, I. The whole empty portion BCD appears uniformly illuminated with a light fomewhat ftronger than that which takes place under the glafs bell, between the two brafs balls. II. When I fend whole difcharges of bottles through the faid fpace, the light appears uniform throughout the fame; but it is more white and more fplendid, in proportion as the difcharges are ftronger; that is, as the quantity of fire that paffes through the vacuum is more copious. III. To this add, that this light lafts only an inftant; and the fhocks thofe difcharges give me, are both ftrong, and like thofe which I fend immediately through my body.

526. Thefe different accidents afford an explanation for the fecond cafe above mentioned; that is, they explain the changes of colour which electric lights exhibit in certain circumftances. Thefe changes depend on the greater, or lefs, rarity of the electric fire: according as it flows with more denfity, it throws a light whiter and more intenfe; in proportion as it is more dilated, it emits a light weaker and more violaceous; and thefe different appearances equally take place in the barometric vacuum, in air that is only fomewhat dilated, and in the atmofpheric air. This fame principle explains alfo the various appearances exhibited by the electric fire in natural phenomena; how it dazzles in a thunderbolt; how it does not offend the eye in an extenfively diffufed flafh of lightning; how it even creates a pleafing fenfation, when exhibiting itfelf under the fhape of a falling ftar; and how it affumes various reddifh appearances in the *aurora borealis*; but of this I fhall treat more largely hereafter, and fhall only obferve here that a few other circumftances befides thofe above, concur in producing thofe variations of electric lights.

527. All the faid accidents moreover prove, that *a complete vacuum may be confidered as a medium pretty exactly deferent; and that this deferency gradually leffens in proportion as the exactnefs of the vacuum diminifhes.* In fact, if I fucceffively introduce air-bubbles into a double barometer, the empty fpace BCD ceafes to be intirely illuminated, but the light gradually gathers itfelf nearer to B and D; and in C, it either becomes very languid, or intirely

intirely ceafes, according as more bubbles of air have been introduced; correfpondently alfo, the lights in B and D appear more vivid, and laft longer; the difcharge is effected with a lefs ftroke, and after it, a greater portion of the charge is found to have remained within the bottle.

528. I fhall have occafion to mention again the reafon why the light of the fire that paffes through an incomplete vacuum, is interrupted in the middle between two deferent bodies placed in it; here I have fufficiently demonftrated, I think, that air oppofes fparks in proportion to its own denfity, and in proportion to the thicknefs of the ftratum through which the latter are led to pafs. The refiftance of air is moreover proportioned to the *width* of the paffage, which the fpark opens to itfelf through it, and a very fimple experiment may prove it. If to the head M (Pl. II. fig. 8.) of the conducting bow MNO, balls of gradually larger diameters are fucceffively adapted, the fpark that will fly to fuch balls, will be larger, will leave a broader mark on the balls, and produce a found of a graver tone. But then it will only leap to a lefs diftance; for the fame ftrength which would be employed by the fpark in driving a longer column of air, were this fpark drawn by a lefs part of a leffer ball, is now employed in expelling a fhorter, it is true, but a *wider* column of air, in confequence of the fpark being drawn, as we fuppofe, by a larger part of a larger ball.

529. If the bodies between which fparks are excited, remain conftantly of the fame fize, then the above proportion between the refiftance of the air, and the width of the paffage which the fpark is made to open to itfelf through it, holds ftill; but here we muft obferve, that *the width of the paffage will rather correfpond with the quantity of the fire that is thrown, and the length of the fame paffage will rather correfpond with the degree of denfity to which the fire is brought.* This is the reafon why a fimple fpark from a conductor leaps to a diftance, at leaft equal to that to which a middling difcharge of a bottle will do; for, though the quantity of the fire thrown from the bottle furpaffes ever fo much the quantity of the fire that leaps from the conductor, yet the denfity in the former is not fuperior to the denfity on the latter; which is evident from this bare confideration, that the denfity of the

the fire in the bottle only proceeds from the denfity of the fire on the conductor.

530. With regard to the proportion that obtains between the breadth of the fpark, and the quantity of fire, Dr. Prieftley has made very elegant obfervations. That gentleman trying with a harpficord the tones of the founds produced by the explofions of various fparks, found (p. 717.) that their gravity correfponded with the extent of the coatings from which the fparks were excited; that is, with the quantity of the fire. Thefe experiments might be carried farther, by placing fmall bits of paper aftride on the ftrings of a harpfichord, and attentively obferving which of thefe would be made to leap higheft by the different explofions of fparks excited from jars of various capacities.

531. Upon the whole, I think that in extenfive ftreams of electric fire, the copioufnefs of it, while it tends to increafe the fpark, greatly contributes alfo to fpread it. I alfo think, that the *deferent* vapours which lightning excites, and as it were fupplies itfelf with, are like a vehicle that greatly ferves to increafe the *amplenefs* of its paffage; but the copioufnefs itfelf of the fire, which at firft partly is thrown at once, and partly fucceeds afterwards, muft undoubtedly alfo contribute to produce the fame effect. From the beginning I obferved, that the effects of fparks increafe, in proportion as the fections of the medium through which they pafs, leffen; and that this increafe of efficiency ought to be imputed to the greater denfity which the fire poffeffes in thofe narrower fections. It is true, that, befides a condenfation of the electric fire, a retardation of the fame muft alfo take place in fuch narrow fections; but if the condenfated ftream of fire be followed by new fucceffive *waves* of it, as we may conclude it to be the cafe, from the uninterrupted blaze and continual fucceffive explofions that take place in lightning, it is evident, that the new fucceeding flafhes muft contribute to promote the motion of the whole ftream.

SECT. II. *On the manner of the action which electric fparks exert on the air.*

532. I think I have been the firft who thought of inveftigating the nature of the action which the electric fire exerts on the air

 through

through which it paſſes. This I have done, firſt in the fifth chapter of *Artif. Elect.* where, from the *ſilence* of the electric fire, when it is merely extracted from, or added to, bodies, and from the noiſe produced by it, when it ruſhes from a point into the air, I concluded that ſparks vibrate the air through which they paſs; and I was thence induced to try, by the means of a tube of glaſs fitted for that purpoſe, what kind of impreſſion the air agitated by ſparks would make on the liquor contained in that tube.

533. The following is the manner after which I uſually make this experiment. I. With ſome cement, or alſo by means of a lamp, I ſeal the end A (Pl. V. fig. 15.) of the tube A B, around the iron wire D R. II. I warm the tube a little, and dip the open orifice B into a glaſs of wine, ſo that it ſucks it in, to the height of one or two inches. III. Into this open orifice B, I inſert another iron wire B U, whoſe end U remains diſtant from the end R of the other iron wire R A, four, eight, or more lines. IV. And in the inſtant when I ſend the diſcharge of a coated ſheet of glaſs through the two wires, the air, ſtruck by the ſpark in the whole interval R U, tranſmits the ſtroke in C againſt the ſurface of the wine, the whole, or part, of which, is driven out at the open orifice B.

534. In repeating this experiment in tubes of various lengths, and with more powerful ſparks, I obſerved, that the part of the ſmall cylinder of wine (I uſe red wine in order the better to diſcern the various accidents that take place) that had been puſhed towards the orifice B, but had not been driven out of it, recovered its former ſtation in S, with a perceivable degree of ſlowneſs. Thence I concluded, that the ſpark, in paſſing through the ſpace R U, introduced a kind of permanent expanſion in the air in it, which did not give way but gradually, and in the ſame manner as an expanſion introduced by heat might have done: I have ſince been confirmed in this opinion, by obſerving, that the length of the cylinder of wine which is driven out, greatly exceeds, when the ſpark is a vehement one, the interval R V, that is the ſpace through which this ſpark paſſes.

535. In *Atmoſ. Terr. Elect.* deſiring to aſcertain whether the air in the above mentioned experiments was driven in every direction

tion, I made a hole with a file in the middle of a tube; then with ſealing-wax I fitted another tube to it, perpendicular to the place where the hole was made, and both thus communicated together through this hole. I made afterwards the perpendicular tube ſuck wine in, to the height of eleven lines; laſtly, I ſealed with ſealing-wax an iron wire to each end of the tranſverſe tube (the two tubes formed together an appearance like that of a T); which extended to the hole of communication. The ſpark being afterwards ſent through the wires; a part of the wine was driven out of the perpendicular tube; and thus I aſcertained that air is driven by ſparks, with a direction even perpendicular to the direction of thoſe ſparks themſelves.

536. I have alſo made ſeveral other experiments of the ſame kind with the former. At p. 78, I mentioned one in which I made the air drive wine into different and contrary directions, from both ends of a tube. In the fig. 16, of Pl. V. a tube A B is repreſented, which is hermetically ſealed at both its ends A and B, around two iron wires, which are brought near each other in C D; perpendicular to the interval left between them, a kind of ſpout of glaſs F is annexed, through which, at the ſame inſtant that the ſpark is thrown, the air blows, and drives a little heap of duſt placed before the ſpout, or ſets in motion a ſmall moveable wheel, and alſo puts out a ſmall light.

537. In fig. 14. four tubes are repreſented annexed to each other, in the ſhape of a croſs. I make each of them ſuck wine in; into two of them I inſert two iron wires, and the ſpark, in the moment that it leaps, drives part of the wine from each of the tubes.

538. In the year 1764, I deſcribed in a ſmall book of obſervations which I ſent to the duke of York, the apparatus which is repreſented in fig. 11. Pl. III. I. The braſs rod D *c*, which riſes from a ſupport D, is inſerted through the bottom of a bottle, and is ſealed to it. II. The rod A *b*, which in A is terminated into a ball, is ſealed within the neck of the ſame bottle, and the two rods are placed at a convenient diſtance from each other. III. To the neck of the bottle, the ſmall tube, or ſyphon *efg* is annexed, which I make ſuck wine in; and when the ſpark ruſhes through

the interval *bc*, it lowers the wine in the part *ef* of the ſyphon, and raiſes it in the other part of it *fg*. It was only in the year 1770, that I ſaw in Dr. Franklin's work, that Mr. Kinnerſley had wrote to him from Philadelphia, a letter in which he deſcribes a ſimilar inſtrument with the above, which he calls an *aereo-electric* thermometer: his letter is dated in the year 1761; that is, is nine years poſterior to the firſt experiments I made on the ſame ſubject. Likewiſe, before I had any knowledge of the above inſtrument, I wrote to the Royal Society of London the deſcription of an experiment in which I raiſed the mercurial column of a barometer by means of electric ſparks. CBG (Pl. V. fig. 17.) is the the tube of that barometer; it is divided in G into two tubes, and the mouth of each of theſe is ſealed, in D and in E, around two iron wires, which remain parted from each other in S; the tube AB is faſtened to the board AB, which riſes from the ſupport HTR. This ſupport is placed near an electric battery, and it communicates, through the wire EF, with the linings of the jars that form that battery; then, touching the coatings with the one end of the conducting bow, and bringing the other end to the wire D, I find that the ſpark which is thrown from S, lowers the ſurface of the mercurial cylinder in B, for one or two lines, or even more, according to the quantity of the charge.

539. In ſhort, in whatever manner the action of ſparks on air, be inveſtigated, every accident that happens concurs to prove, I. *That air is driven proportionally to the copiouſneſs and denſity of the electric fire.* II. *That it is driven in every direction.* III. *That it is driven by a kind of dilating force which does not inſtantly ceaſe, but is ſomewhat continued, and is like to the dilatation that takes place in it, when a new degree of heat is introduced into it.*

SECT. III. *On the extenuated ſhape of ſparks.*

540. I have not as yet read that any body has obſerved a particular kind of appearance, which pretty conſtantly takes place in the ſhape of the electric ſparks that are drawn from a conductor: theſe ſparks appear more ſplendid, more vivid, of a whiter and intenſer

tenſer light near their extremities, than in their middle, where they appear of a red violaceous colour, more languid and narrow; and the place where the light thus grows languid and narrower, is uſually moſt diſtant from that of the two bodies between which the ſpark is thrown, which is moſt convex, or ſharp, and neareſt that body, which is leaſt convex, or flatteſt. Thus, when I bring the knob of the braſs cylinder (516) near the conductor, that part of the *extenuated* ſpark which I mention, remains the nearer the conductor, in proportion as the latter is leſs convex than that knob.

541. This phenomenon has appeared to me worth conſidering attentively, either on its own account, or becauſe it may ſeem at firſt ſight to favour the doctrine of the *affluency* of two diſtinct electric fluids, which lighten when ſeparated, and vaniſh from ſight when they join together. Being at a loſs how to reſolve this difficulty, I have undertaken to examine the paſſage which ſparks open to themſelves through a thick quire of paper, and have found that this *paſſage* alſo appears extenuated in its middle, and exhibits ſome likeneſs with the ſhape of the ſparks which paſs through the reſiſting medium, air.

542. Diſcharging, therefore, at ſeveral times, a coated plate of glaſs, through a maſs of ſix ſheets of paper ſewed together, I conſtantly obſerved, I. That the edges of the holes made in the upper leaves by the ſparks, were bent upwards; and in the lower leaves the edges of the holes were bent downwards. II. That in thoſe *upper* leaves which were at top, and lay near the conducting bow, the edges of the holes rent in them, were both larger and more bent upwards, than in the leaves next under them; and correſpondently, the holes were broadeſt, and the edges of them moſt bent downwards, in thoſe *inferior* leaves which lay neareſt to the coating of the plate. III. In thoſe leaves which lay about the middle, the holes were ſo narrow as to be hardly diſcernible, and the extremely ſmall bur which was around them, ſeemed, in the leaves on the one ſide, to be raiſed upwards, and in the other leaves to be thrown downwards.

543. But we muſt obſerve, that in order to obtain this gradation, both in the ſize of the holes, and the extent of the bur around them, the quire of paper muſt be uſed juſt as it is taken

from

from the ream without opening its leaves, and muſt be placed ſoftly on the coated plate, without preſſing upon it, otherwiſe the above gradation in the ſucceſſive holes will not obtain. If the quire remains with its loweſt leaf cloſely united to the coating of the plate, the width of the hole, and the bur around it, will both be leſs; and if the leaves in the middle remain at any diſtance from each other, the holes made in them will be larger, and the bur be directed towards that part where the ſeparation takes place: theſe obſervations have ſuggeſted to me the following experiment. I. I part the leaves of the quire into two diſtinct ſeries; that is, I place a fold of eight leaves between the ſixth leaf and the ſeventh. II. At another time, I part the leaves into three ſeries, putting the ſame diſtance between them as I did between the two former. III. Again, I part them into four ſeries; and, in all theſe caſes, the holes made by the ſparks keep the following rules. I. The widening of the holes, and the direction of the bur around them, always proceed from the middle of the leaves that are united together, towards the places of ſeparation, with contrary directions. II. Meanwhile the differences between the ſizes of the holes grow leſs, as does alſo the bur around the holes, in proportion as the number of the partitions between the leaves has been increaſed.

544. From the conſtancy of thoſe facts, it reſults firſt, that the reaſoning of thoſe who conclude from the burs around the holes being turned outwards in contrary directions, that there exiſt two diſtinct contrary ſtreams of electric fluids, is ill grounded. Firſt, according to that hypotheſis, ſuch ſtreams cannot be ſuppoſed to meet each other within the leaves, but muſt ruſh from the middle of them towards oppoſite parts, conformably to the direction of the bur; and, ſecondly, our laſt experiment makes it alſo neceſſary to ſuppoſe as many *pairs* of ſtreams of the electric fluid, as there are partitions made in the quire of paper, which is an abſurdity. If it were inſiſted upon, that the ſtreams of electric fire, inſtead of ruſhing from the middle of the leaves, meet there, the ſame abſurdity from their multiplicity ſtill exiſts; and, beſides, there would be a downright contradiction to facts, becauſe in ſuch caſe the bur would be bent inwards, towards the middle of the leaves, where ſuch ſtreams, as it is ſuppoſed, meet.

545. On

545. On the contrary, the following is a very ſimple, as well as a neceſſary principle, viz. *that the electric fire tends to explode, in every direction, the parts of the reſiſting ſubſtances through which it paſſes.* I availed myſelf of that principle (*Art. Elect.* p. 136.) to explain the common experiment in which the bur around the hole rent by a ſpark in a piece of paſteboard is always thrown outwards on both ſides; now, I ſhall proceed to deſcribe the ſeveral accidents that take place in my new experiment of the *partitioned* quire; only, I ſhall add to the above principle, what indeed is a neceſſary conſequence of it, viz. *that the actual exploſion of reſiſting ſubſtances into various directions, is in an inverſe ratio to the reſiſtance, or force, that ſupports thoſe ſubſtances in thoſe directions.* If we meant to expreſs this principle more clearly, we ought to ſay, *that the electric fire diſplaces the parts of the bodies it meets with, not by ſtriking againſt their ſurfaces, as ivory or chalk balls do in experiments on the colliſion of bodies, but by traverſing, that is, by penetrating, through the pores of ſuch bodies, and by exerting in its paſſage its expanſive force, condenſating and driving forwards what fire it finds inherent within thoſe bodies, and experiencing, at the ſame time, a re-action from them equal to its own action.*

546. Theſe principles being now laid down, we may eaſily conceive, I. That the ſpark, at the inſtant that it forces its way into the internal pores of the whole united quire, muſt, in conſequence of the equal reſiſtances oppoſed to it, both by the ſix upper leaves, and by the ſix inferior ones, muſt, I ſay, burſt the former upwards, and the latter downwards, ſince the thick ſubſtance of the paper around the holes does not allow it to produce any lateral effect. II. The inſide leaves will reſiſt more than the outward ones, becauſe their burs, when driven either upwards or downwards, muſt needs get into the holes of the next leaves; and conſeqently cannot attain the given width of thoſe holes, but by diſtending and opening them ſtill more. III. Whence it follows, that in the inſide leaves, the ſpark muſt grow continually narrower, and open narrower holes, ſince the reſiſtance it meets with continually increaſes.

547. From theſe obſervations it follows, that in every ſeries of leaves joined together, whether there be one, two, or three

ſuch

ſuch ſeries, the ſame phenomena muſt reſult in each of them ſeverally, that take place in the whole compact quire; with this additional circumſtance, however, viz. that according as the leaves will be fewer, the difference between the internal and the external holes will be leſs, ſince the external holes will, in ſuch caſe, be diſtended by fewer ſucceſſive burs; and reciprocally, the internal holes having fewer external holes within which they muſt diſpoſe and lodge their own bur, will the more readily open and widen.

548. Meanwhile, ſince the exploſion in contrary directions, which in the *united* quire is ſingle, becomes multiplied in the *partitioned* quire, in conſequence of the intervening of ſufficiently thick ſtrata of air, it plainly appears, that it is the united effect of the ſucceſſive reſiſtances which take place, that makes the ſpark act back againſt the former half part of the ſame.

549. I take another compact quire from the ream, I ſew it in the ſame manner as above, and between the ſixth and the ſeventh leaf, I now inſert a ſheet of tin, like thoſe uſed for looking-glaſſes; and I find, that the bare interpoſition of this thin deferent ſubſtance, is ſufficient to cauſe the bur around the holes, in each of the two ſeries of leaves, to be directed into two different and oppoſite ways; the three firſt leaves of the *upper* ſeries have their bur directed upwards, and the three others have it directed downwards; the ſame takes place in the *lower* ſeries, only the ſpark uſually divides itſelf within it, into two or more ſparks. It is, however, neceſſary to obſerve, that the holes that took place near the intervals filled by air, in the former experiment, were ſmaller than thoſe which were rent near the head of the conducting bow, or near the coating of the plate; but the holes which are rent cloſe to the tin ſheet, in this experiment, are equal to the external ones, with reſpect both to their width, and the extent of their bur; this is becauſe the ſpark can more freely expand itſelf through the deferent metal, than through the intervals filled by the moveable indeed, but *cohibent* air, and the tin ſheet better interrupts the continuation of the reſiſtances from the ſucceſſive leaves of paper, than a ſtratum of air can do.

550. In the above experiment with a metallic ſheet, we may obſerve how, to the particular action of the ſparks againſt the

quire

quire of paper, correfponds a particular reaction of the refifting quire againft the fparks, in confequence of which, the latter remain particularly narrow and condenfated; of this a proof may be feen in the above tin fheet itfelf, which is more completely melted by the fpark, when it is traverfed by it within the quire of paper, than when it is traverfed in the open air.

551. But to return to our principal point; when I firft examined the kind of contraction that takes place in fparks when paffing through air, I thought it was to be explained in the fame manner as the increafe of contraction that takes place in them, when fent through quires of paper; but confidering afterwards the fubject more attentively, I could not be fatisfied, and propofed to myfelf the following queftions: I. Why fhould not air yield equally throughout its whole fubftance? furely the conftituent particles of it are exceedingly moveable, and entirely free from any fuch adherence to each other, as obtains between thofe of paper. II. Ought not fparks, when contracted in air, to acquire a greater denfity, and of courfe a more vivid light, in the place where fuch contraction obtains, than in any other; in the fame manner as it happens within the quire of paper, where the fparks, by condenfating themfelves, more completely melt the fheet of tin than in the open air? III. Does fuch contraction proceed from the fuperior rapidity with which fparks rufh, when in the middle of their paffage; in the fame manner as rivers grow narrower when their velocity increafes? and is fuch rapidity apt to prevent the fire from making any ftrong impreffion on our eyes?

552. This I can certainly affirm, *that the electric ray that paffes through air extremely dilated, does not fuffer any contraction like that which fparks fuffer when they pafs through atmofpheric air*; *nor does that ray fuffer any weakening of its light in its middle.* The ray which in a complete vacuum paffes from the ball B to the ball C (Pl. II. fig. 9.) an inch diftant from the former, is perfectly continuous, and equable in its whole length: its light indeed is much inferior to that of fparks, but then it is uniform throughout; therefore, it is evident, that the contracting and weakening of fparks in their middle, whether apparent, or real, muft be attributed to

ſome particular action exerted on them by the air through which they paſs.

553. And it is in that view, that examining more exactly the light, which I have called of *overflow* (142.) I found that it is not to be imputed ſolely to the reaction of the electric fire in the ball C, into which the electric ray B C ruſhes, as I have ſuppoſed in num. 146; but this light (which is repreſented in the fig. 10, 11, and 12, pl. V.) is proportioned to the remaining quantity of air within the receiver, as well as to the quantity of the fire which paſſes through it; ſo far, I mean, as it reſults from the reaction of this ſame remnant of air, independent of the reaction of the electric fire in the ball, into which this fire paſſes.

554. I think, however, that two conſiderations of the ſame kind, as thoſe with which I explained the light of *diffuſion*, and that of *overflow* in dilated air, will alſo ſerve to explain the preſent difficulty, that is, why ſparks in atmoſpheric air, are larger at both their extremities than in their middle. If a ſmall quantity of fire iſſuing from the ſurface of a ball, is retarded, and in ſome degree kept back to it by air much dilated, ſo that it ſhines around the ball from which it proceeds, why ſhould not atmoſpheric air, which is much denſer, alſo repreſs and keep back in ſome degree the copious fire of ſparks, when this fire ruſhes againſt it? This ſeems at leaſt to be a neceſſary conſequence of the ſame power in air, which cauſes the accumulation of electricity around deferent bodies, previous to its leaping from them, under the ſhape of ſparks (553); and the minute ſparks which conſtantly precede the darting of the main ſparks, confirm this conjecture.

SECT. IV. *On the crooked direction of ſparks acroſs air.*

555. I flatter myſelf that I have, in num. 516, 517. ſufficiently explained the cauſe, and laid down the law, of the *continued incurvation* of ſparks, which from the ſurface of one body run to another body preſented to it, in a ſituation almoſt parallel. This cauſe may be expreſſed in univerſal terms; *whenever a ſpark,*

being

being determined by the direction of its ſtream to move in a right line, meets with a reſiſtance from air, it deviates from its former courſe towards ſome other place where it finds leſs reſiſtance. This is the ſame principle of which I made uſe in my firſt Work on Electricity, in order to explain the mutual divergence of the rays that form electric bruſhes. Preſent, I ſaid, your finger laterally and ſomewhat underneath, to the electric bruſh which ſprings from a very ſharp point, and you will ſee it bending and lengthening itſelf towards the tip of your finger, with a continued incurvation; the reaſon is, that as the ſtratum of air through which the rays of the bruſh endeavour to paſs, is leſſened by the approach of your finger, which prevents their deviation from each other, as well as their deperdition, and cauſes them to ſtretch and gather towards your finger.

556. And would not this principle, that air reſiſts more in proportion as the thickneſs of its ſtrata increaſes, beſides the common curvature of ſparks, alſo explain the complicated motions of certain ſparks, which appear as if they were broken, and generated by repeated reflections? The French electricians call them *zig-zag* ſparks; we might as juſtly call them *capricious* ſparks, ſince they change ſo ſuddenly their directions, that they appear to us, who are ignorant of the cauſe, as if prompted by kinds of whims. We might alſo call them *hooked* ſparks, ſince their ſucceſſive flaſhes are often inclined to each other, at very acute angles, like hooks. In order to inveſtigate whether there is any regularity in theſe motions, ſeemingly ſo irregular, I ſhall lay before the reader ſeveral obſervations which I have made on this ſubject.

557. Firſt, I have obſerved, that with a ſeries of deferent bodies properly diſpoſed, I may eaſily render the direction of a ſpark as crooked as I pleaſe: it was in this manner that I repreſented, in num. 539 of *Natur. Elect.* the *zig-zag* direction of lightning; that is, I made a ſpark paſs along a ſeries of ſmall metallic balls placed near one another in a crooked line, on a cube of wax. At other times, proceeding on the ſame principle, I made the ſpark paſs in the middle through a ſeries of corners of metallic ſheets, ſo diſpoſed as to make the ſpark repreſent in its way letters, branches of trees, &c. But now, though it be a fact, that

ſparks will ruſh to ſuch deferent bodies as lie in, or near, their way, yet the ſuppoſition of a fortuitous diſpoſition of deferent bodies, does not completely account for the particular mode of the direction of thoſe ſparks which we have called *zig-zag* ſparks ; the above mentioned motions of ſparks towards deferent bodies, conſtantly appear to be only continued progreſſions of the ſame towards a given point, through the leaſt reſiſting medium ; but their *zig-zag* motions ſeem as if they were the effects of reſiſtances that actually repercuſs thoſe ſparks, and throw them back.

558. Secondly, I obſerve, that it ſeems that the intervention of deferent bodies, rather hinders than favours the zig-zag direction of ſparks. When I preſent a deferent body laterally to the paſſage of a ſpark, the latter *zig-zags* leſs than it otherwiſe would do. Lightning, when it ruſhes through the body of a cloud, has, at moſt, a ſlight flexure in its direction, and no zig-zag takes place in its motion; the deferent cloud affording to it a paſſage continued enough for it to run in a ſtrait line, or nearly. Flaſhes which ruſh through bodies of clouds, exhibit the appearance of long rectangular bands; and they ſeem to *zig-zag* in thoſe places only where they ruſh through tracts of air that are free from any great quantity of deferent vapours. I often diſcharge the coated plate AB (Pl. VI. fig. 9.) through a drop of mercury, which I place on the naked margin HC; and I find afterwards that the paſſage of the ſpark is marked by the effluvia, or fumes of the mercury, which the ſpark has left imprinted on the plate; this track indeed is ſomewhat tortuous, but leſs ſo than that of ſuch ſparks as paſs directly through air, without the aſſiſtance of any vehicle.

559. As a confirmation of this principle, that ſparks and lightning zig-zag moſt when they paſs through air free from vapours, I obſerve, in the third place, that either in the air-pump, after the air has been accurately drawn off, or within the pretty large balls of my barometer, ſparks never have the leaſt zig-zag appearance: it is true, I ſpoke, in num. 283, of flaſhes of fire which moved with a flexuous motion within a glaſs-bell emptied of air; but I obſerved, at the ſame time, that theſe flaſhes flowed cloſe to the inner ſurface of the bell (284), and that this was owing to the tortuous

tortuous difpofition of fome parts of that furface, which being lefs *refifting* than the reft, invited the fire thus to wind clofe to them. I even can, at pleafure, produce fuch a flexuous motion of the electric fire as I mention here; I raife the rod A B (Pl. IV. fig. 12.), fo that it remains at a confiderable diftance from the bafon; then, with my wetted finger, I defcribe a crooked line on the glafs-bell, which, from the brafs armature of its neck, reaches down to the bafon; and I immediately fee that the electric fire begins to wind about, clofe to the inner furface of the bell, correfpondently to my finger; this fire being now enabled to adhere and accumulate itfelf on this inner furface, becaufe it now can, through the deferent dampnefs affixed to the outfide of the bell, drive fome fire from that outfide. But when the ray within the bell is, as it were, left to itfelf, and the air is accurately drawn from the bell, then this ray runs between the two balls B, C, (Pl. II. fig. 9.) in a direct line. If the two balls be removed to a greater diftance from each other (Pl. IV. fig. 6.), and the remnant of air be fomewhat confiderable, then the ray will indeed fail in its middle, and divide at its extremity, but its zig-zag motion will only begin to take place when a very confiderable quantity of air has been let into the receiver. Now, do not thefe facts render it manifeft, *that the zig-zag directions of fparks chiefly arife from the reaction of the air, againft them?*

563. But how can we afcertain the precife degree of this reaction, which is neceffary to begin to produce the zig-zag motion of fparks, fince their continuous incurvated direction has been proved to be alfo the effect of this reaction? With refpect to that, I firft obferve, that in electric brufhes, there is not the leaft appearance of a zig-zag motion; now, how does it come to pafs that the fparks which form thofe brufhes, thus rufh through a long tract of air, and yet are never thrown back, but only continuoufly incurvated?

561. To this obfervation we may add this other, which is an exception to the former, viz. that fparks which move with incurvated directions, are not, however, always without fome appearance of a zig-zag motion. Thofe fparks which, from the conductor, leap to the knuckle of my finger, or to the head of a cylinder

cylinder presented to the conductor, in a situation almost parallel to it, always exhibit some *zig-zag* motion mingled, as it were, with their *incurvated* progression; as for brushes, they constantly bend themselves towards my finger, in a regular manner, and without any tortuosity, only the nearest rays bend themselves most; those which are farthest, least of all.

562. *It appears that it is the rapidity of the blow struck by sparks against the air, which causes the latter to stop their course, turn them, and repercuss them*; in the same manner as the rapidity of the blow of a stick against water, causes it to jump back; or, in the same manner as water put in a pistol, and shot out of it, is repercussed back by the air it strikes against.

563. In fact, I observe in the sixth place, that such sparks as are thrown with a greater force than others, and are consequently enabled to rush through longer tracts of air, also exhibit more complicated zig-zags, and deviate from their right course through longer spaces and with longer flashes, than others do; this is rendered evident, by observing the different directions of lightning, and even of electric sparks when their intensity is increased.

564. It appears besides, that air, in consequence of its moveableness, is peculiarly apt to repercuss sparks, and turn them out of their right course through *hooked* directions. My reason for thinking so, is, that sparks sent through mediums more compact, but then of another nature than air, assume no such hooked motions. Once, in very favourable weather, I sent the discharge of my three large crystal jars through six quires of paper stitched together; and, examining them afterwards, I found that the long hole made through them was in a strait line, that is to say, *the great lateral resistance of the paper had kept the spark in its strait course; whereas, the moveableness of the particles of air, allows sparks to run through different tracks of the same, when repercussed in consequence of the violence of their own motion.*

565. Lastly, it has appeared to me, that such sparks as obliquely rush against air, exhibit a still more complicated hooked direction than others do; the reason of which, I think, is, that as the stream of fire strikes unequally against air, it is more easily

turned

Tav. X

turned out of its right courfe towards that place where it ftrikes with the leaft force, than towards any other. The truth of this obfervation I commonly inveftigate by means of the following experiment: I. I adapt to the conductor Y (Pl. X. fig. 1.) a brafs rod AB, two lines thick, and rounded at its extremity. II. On the fupport of glafs U, I faften horizontally another fimilar rod C D, which is likewife rounded in C; I difpofe this fecond rod in fuch a manner, that it be fome few lines lower than the end B of the rod A B, and its end about fixteen lines diftant from it. III. Then, as often as the conductor Y has again received a charge, I prefent my finger to the end D of the rod C D; when fparks take place in B C, which are the more crooked, as their paffage is rendered longer. Thefe fparks are ufually thrown from B with a kind of a downward direction, but they immediately bend their courfe towards the other rod, and after this firft curvature, which is rather of a continuous kind, their direction becomes remarkably crooked.

566. I fhall here propofe a queftion; why does the fpark after being repercuffed towards B, yet continue to move towards C? This, no doubt happens, becaufe in A B there is always a remnant of excefs, which preferves a degree of tenfion in the fire of the ambient air, and the confequence of this tenfion is, that the fpark is again repelled; in fact, we fhall foon fee, that electric brufhes, which may with truth be confidered as aggregations of infinitely fmall fparks, are repercuffed and made to deviate, when they meet with *redundant* atmofpheres.

567. To which add this other confideration, viz. that the redundant atmofphere around the rod A B, and much more fo, the efficiency of the fpark itfelf when it leaps, always drive fome natural fire both from the furface of the rod C D, and from that of the air contiguous to it; fo that the tenfion of the natural fire in the air immediately contiguous to the rod C D, becomes leffened, and this air thus oppofes lefs refiftance than any other place does, to the paffing fpark; thus we have feen, that brufhes always deviate towards deficient atmofpheres (115).

568. I once flattered myfelf, that I could be able to direct, at my pleafure, the paffage of the fpark BC (Pl. X. fig. 1.) by means of the atmofphere of a charged bottle; but I perceived, that fuch

atmofphere

atmofphere, by its increafing the contrary electricity in the rod C D which I touched, ferved more to favour the direction of the fpark towards C, than to make the fame move in zig-zag. When I held the bottle pretty near the middle point, between B and C, then the fpark, whether I grafped the belly of the bottle, or held the fame with an infulating handle, leaped to the hook, concealed itfelf in it, and thence leaped to the head C of the other rod. From the interpofition of the deferent hook, the fpark indeed was enabled to run through a longer tract than it would otherwife have done, but then it had a lefs crooked direction than common, in each of its two partial paffages. When I grafped the belly of the bottle, the fpark leaped to a ftill greater diftance, and the bottle at the fame time was difcharged; it feemed as if the hook of the bottle, by its more intenfely introducing a contrary electricity in the rod C D, quickened the paffage of the fpark from B; and reciprocally, the paffage of the latter quickened the difcharge of the bottle.

SECT. V. *On the divifion, and on the union of fparks acrofs air, and particularly on the divifion with circles and rings, obferved by Dr. Prieftley.*

569. *If a fpark, in the act of its paffing through a refifting medium, meets with a deferent body, which, not being of itfelf fufficiently continued, yet is of fuch a nature, that the fpark may explode it into a number of particles, fo that the fum of the feveral intervals that will afterwards take place between thofe particles, oppofe a lefs refiftance than that oppofed by the fpace of air which the fpark muft otherwife run through, the fpark will actually explode that body, and pafs, divided, through its particles.*

570. I demonftrated the reality of this law, by placing a fheet of tin in the middle of a quire of paper. I. The fpark bores a fingle hole in thofe leaves which lie between the conducting bow and the metallic fheet. II. But in thofe leaves which lie between that fheet, and the ample furface of the coating of the plate on which the quire is placed, it almoft always divides itfelf into two, three, or more fmall fparks, each of which opens a particular paffage to itfelf through the quire. III. When I afterwards carefully

fully examine thofe feveral holes that have been made, I find that they have been made in thofe places when the leaves, either on account of their thinnefs, or lefs compact texture, oppofed the leaft refiftance; and that in thofe places where the largeft holes were made, fuch circumftances plainly appeared to have taken place, as muft have made the leaves to yield more readily than in any other part. IV. So that I have at times amufed myfelf in marking out before-hand the places of the holes, either by boring, or by rafping, or wetting, fome of the leaves. In order to make the above experiment, I ftitch, as I mentioned before, the quire of paper; but then it is neceffary to faften the tin fheet with wafers, elfe the fpark is fure to pafs through the holes of the ftitches.

571. When the quire of paper has been placed on that furface of the coated plate which is charged by excefs; then, at the approach of the conducting bow, it is no longer a fpark that divides itfelf, but a number of fmall fparks which, from the fheet of tin, gather and unite towards the head of the conducting bow: and the caufes and laws of thefe *unions* of fparks are reciprocally the fame with thofe of their divifions.

572. Nor is it abfolutely neceffary, in order that the fpark may divide itfelf, that the tin-fheet fhould be inclofed in a quire of paper. Once, difcharging a battery of three large jars through a tin-fheet, I obferved that it was not only melted in that place through which the main fpark had paffed, but that many lucid points had taken place around the main hole; and among thefe, one had been melted quite through, fo that the fheet was bored in that place. Sending, at another time, a like difcharge through a fimple leaf of paper, I obferved that befides the principal hole rent in it, there were feveral other very fmall ones; though they were difpofed without any apparent order.

573. Hence I concluded, that fparks are aggregations of many other fmall fparks, and that the copious fire of which the larger fparks are formed, throws around them a number of fmall ones.

574. It was only after reading the experiments of Dr. Prieftley, that I obferved the circles and rings that take place on balls of

glafs broken by fparks. That Gentleman was the firft who, drawing fparks from a battery of forty fquare feet (p. 699.), found that the main fpark divided itfelf into a number of fmall ones, obferving the following order. I. Some of thefe fmall fparks bored a number of fharp cavities in the glafs, the aggregation of which formed a circular fpot. II. Other fparks bored around the faid fpot, other cavities that formed a circle, or ring, concentric to it. III. On tin fheets, as they were more eafily melted, an additional concentric ring took place. IV. On the compound fubftance mentioned by Dr. Prieftley, which diffolves in boiling water, a third ring was to be perceived. V. The cavities in the middle fpot were deeper (to me it has appeared that they are alfo larger) than in the fucceffive concentric rings. VI. In fuch metals as are eafily melted, the cavities were deeper. VII. The circular fpot, and the rings made on different metals were equal, only fome fmall difference in their fizes, arofe from the difference in the fizes of the cavities, by which they were refpectively formed: I will add here, that I have alfo obferved, that intenfe fparks (and much more fo ftrokes of lightning), melt a much more confiderable part of the furface of a fheet of lead, than of a fheet of tin. Dr. Prieftley has made fo many other experiments, that he has greatly confirmed my conjectures with regard to the production of *inchanted* circles (See *Atmof. Terr. Elect.* pag. 215.) I will endeavour to repay the fervice by trying to explain the nature and caufes of thofe circles which he has difcovered.

575. I. Though the intenfe fpark fent by Dr. Prieftley feems to leap in an indivifible inftant, yet its parts in reality fucceed one after the other. II. The firft part of it rufhes, I think, from that part of the convexity of the conducting bow which lies neareft to the coating of the plate, and produces the middle circular fpot; but as this fame part, by leaping firft, ftrikes againft the air with prodigious rapidity, and as the latter cannot immediately give way all around, there refults in the fire a momentary increafe of denfity. IV. The remnant of the fpark, which is kept back by the foremoft portion of it proportionally to the refiftance which it meets, is refufed admittance by the ftratum of condenfated air which furrounds this foremoft portion, and is thus led to pafs

paſs through the next ſtratum; whence reſults the firſt ring around the circular ſpot: an explanation of the ſame kind may be given for all other additional rings that may take place around the former.

576. From the rapid ſtroke of ſparks againſt the air, ſonorous undulations of the latter muſt needs ariſe; hence the loud cracks which ſtrong ſparks always produce.

577. Dr. Prieſtley has alſo ſent intenſe ſparks, like thoſe above mentioned, through air brought to a denſity double to its common degree, and found that the circles were both leſs diſtinct and narrower, than thoſe made in atmoſpheric air. Air, when denſer, muſt needs oppoſe a greater reſiſtance to ſparks, and thus deaden, as it were, their efficiency; hence I have found, and I think Dr. Prieſtley has obſerved the ſame, that the above circular impreſſions were deeper when the ball of the conducting bow was in contact with either of the coatings, than when both balls were kept at ſome diſtance from them, and the ſparks thereby obliged to run through ſome conſiderable ſtratum of air; it indeed ſeems, that a denſer ſtratum of air weakens a ſpark in an equal degree, with a higher ſtratum of the ſame.

C H A P. II.

On ſparks, relative to water, and other liquids.

578. I Think that the effects of the electric fire on water, may be reduced to the following principle: I. *The pores of water are of leſs capacity for conducting the electric fire than thoſe of metals.* II. *Electric fire not only tends to ſeparate the particles of water, and to ſcatter them into vapour, as common fire does; but it moreover effects this with a rapidity proportioned to the peculiar rapidity with which it moves.*

579. From theſe principles it follows: I. *That electric fire tends to propagate itſelf chiefly near the ſurface of water:* the reaſon is, that, ſeeking to diffuſe itſelf through the paſſage of leaſt reſiſtance within the water, it muſt move near its ſurface, becauſe the par-

 ticles

ticles of the water will there refift lefs againft being feparated, and its pores againft being opened. *According as the electric fire will be obliged to pafs through water, at a greater depth* under its furface, the weight of the column which it muft then raife, will increafe, *and of courfe alfo the refiftance, which it muft furmount to force open the pores of the water.*

580. It follows, in the fecond place, that the electric fire when compelled to pafs through a body of water, will exhibit various phenomena, according to the amplenefs and the length of its paffage, and alfo according to its own quantity and denfity. *Let the body of water to be traverfed, be both narrow and long; it will obftruct the paffage of the fire proportionally to thofe two circumftances;* the reafon is, becaufe in confequence of the narrownefs of the body of water, its pores will be proportionably fewer; and fince the refiftance of any medium increafes, as I have before demonftrated, in proportion with the length of it, the refiftance oppofed by the above body of water, will alfo increafe in proportion with its length; hence it follows, that a given quantity of fire will, through a narrow and *long* body of water, pafs divided, and with difficulty.

581. Let the above body of water remain of the fame length, but let its width be increafed; then the number of the deferent pores in it will increafe proportionally, and the refiftance oppofed by it alfo proportionally decreafe; therefore, the fire will pafs through it more inftantaneoufly and more compact, than formerly.

582. Let the length of the furface of the water be leffened, the refiftance arifing from this length (which even in metals is pretty confiderable), will alfo be proportionally leffened; confequently the divifion of the fire will alfo be lefs than formerly, and it will pafs more united and compact. If the length of the body of water is leffened beyond a certain degree, fo that the quantity and denfity of the fire may divide it with fufficient violence, and fcatter it into vapour, this will increafe the union of the fpark in a peculiar manner: the reafon is, becaufe fuch vapour will, in an inftant, expel the ambient air, and effect a kind of vacuum through which the fire will rufh the more united and compact; which, at firft fight, appear repugnant to the former facts: thus, a little moifture fpread over a fpace of a few inches, will conduct in a very

compact

compact united manner, the ſame diſcharge of a battery as would, if ſent through a fur of water nine inches long, grow divided, and produce in its paſſage a rattling, or noiſe more or leſs continued.

583. But in all theſe phenomena ſeveral various accidents will be complicated with the former, according as the quantity and denſity of the fire which will be ſent through the water, ſhall vary. A quantity of fire that is only adequate to the capacity of the pores of a given body of water, will paſs through the water without ſenſibly diſtending its pores, without lightning, without cracking, and without being ſenſibly retarded. But more denſe and more copious fire, in proportion as it will exceed the capacity of thoſe pores, will be proportionally more retarded, will diſtend more pores, will emit a more vivid light, and produce a briſker cracking; nay, it may produce all theſe effects, or even explode the water into vapour, through the body of water may be rendered even longer and ampler than formerly.

584. The above obſervations will account for all the different phenomena exhibited by the electric fire, in his paſſage through water, or other liquids. Endeavouring, in the chap. VI. of *Artif. Elect.* to confute the common opinion of thoſe philoſophers who compared the *conducting* property of water to that of metals, I obſerved firſt, that a tube of glaſs, freſh taken from the dew, tranſmitted only weak ſigns of electricity to a Chain, when compared to thoſe tranſmitted by the ſame tube, when a very narrow ſtripe of metal had been paſted to it. Likewiſe, when the ſtrings with which I ſuſpend my conductors, are in any degree damp, or when the ambient air is damp, I ſtill continue to be able to draw ſparks from the conductors; nay, though the roſin cake, on which the FRANKLINIAN iron rod is placed, may have become intirely wet, yet the rod ſtill continues to afford ſtrong ſparks.

585. That is to ſay, the very thin ſuperficies of dewy moiſture, which furred the long ſlender tube of glaſs, could only conduct little fire to the Chain; the damp ſtrings, and the damp air, could not diſſipate the whole fire raiſed on the conductors; and a great part of the copious quantity of fire which, from the

clouds,

clouds, rufhed into the FRANKLINIAN iron rod, could not be difcharged through the thick fur of water that covered the rofin cake under it; all which was evidently owing to the pores of water being of a much lefs capacity for conducting electric fire, than thofe of metals.

586. In the following chapter I continued the fame experiments by attempting to difcharge a coated plate through water. I. Through a thin ftratum of water, fpread on a plate of glafs two lines wide, and fix inches long, the fpark paffed divided, with a continued rattling, and produced a light that might eafily be perceived in the dark. II. The water contained in a veffel two inches wide, and fix inches long, tranfmitted the difcharge to me, with a very weak fpark. III. The ample furface of the water contained in a common difh, tranfmitted to me a ftronger fpark. IV. When I repeated the experiment by fending the difcharges through my body, I was ftruck by them proportionally to the *union* of the light and of the fparks; a difcharge with bare *rattling*, without fparks, gave me no fhock; fuch difcharges as fparkled more or lefs, ftruck me accordingly.

587. Now, if thofe experiments are combined with thofe made on lakes and rivers, by Meffrs. Jallabert, Monnier, and Franklin (which I have repeated on the furface of the Po), and in which ftrokes pretty confiderable were always obtained, we may conclude this, that in the above mentioned ftratum of water two lines wide, and fix inches long, the total capacity of the pores apt to conduct the electricity was extremely fcanty; hence the fire became much divided, and produced a rattling noife, and fome light, as it diftended the fuperficial pores of the water; for rattling and lightning always takes place when the electric fire difplaces and drives the particles of any refifting medium. But in the fmall veffel above, in the difh, in rivers, the difcharge paffed lefs divided, in proportion as the deferent ftrata of water were become more numerous; that is, in proportion as the refiftance it met, was become lefs.

588. In the fame chapter I proceeded farther, and attempted to difcharge a coated fheet through water inclofed in a flender tube of glafs fix inches long, and the third part of a line wide; and I never

never could obtain either ſparks, cracks, or light. I inſerted braſs wires into the tube, and fixed them at a little diſtance from each other, but it was only when the diſtance between them was made ſo ſhort as the third part of a line, that the diſcharge was able to paſs intire and compact, and produced a loud crack, though leſs ſo than one produced by a diſcharge ſent through the open air. I. In the interval between the two wires, I ſaw a vivid light take place. II. The tube was broken, and a piece of it ſeparated, which was half an inch long, and correſponded to the two wires. III. The two main pieces of the tube were flung in oppoſite directions, and the ſhatters were flung beyond them.

589. To underſtand the above facts, we muſt conſider, I. the narrowneſs and length of the body of water that was incloſed in the tube, and the degree of ſtrength with which the glaſs tube ſupported it. On account of the narrowneſs of the cylinder of water, there were in any ſection of it made perpendicular to its axis, only a few pores, and theſe too of a ſcanty capacity for conducting the electric fire. The length of this ſame body of water alſo proportionally increaſed the reſiſtance of it; becauſe in each ſection into which we may conceive it to have been divided, an additional reſiſtance was oppoſed to the ſpark, which ſought to paſs undivided: farther, the ſupport from the tube in which this water was contained, did not allow its pores to be ſuddenly diſtended, but with difficulty. Now the concurrence of all thoſe impediments muſt needs have cauſed the diſcharge ſent in the firſt inſtance through the cylinder of water, either to have failed, or to have only been effected in a very divided manner, and without affording either crack or light. II. But when the metallic wires were brought to a ſmall diſtance from each other, then the diſtance to be ſurmounted being thereby proportionally leſſened, the ſpark diſtended the pores of the water with a force ſimilar to that of the common fire, though with an incomparably greater rapidity; and the water flying into a vapour highly elaſtic, ſhattered the tube to pieces.

590. The reaſon why a ſpark can paſs through a cylinder of incloſed air much longer than any one of water, without ſhattering the

the tube, is owing to the *compreſſibility* of air being ſo much ſuperior to that of water *), and alſo to the prodigious violence with which water flies into an elaſtic vapour. Dr. Franklin repeating this my experiment with ink (p. 145) could not find the leaſt ſtain left by it, on a ſheet of white paper on which he had placed the tubes. One day making uſe of wine inſtead of ink, I found the braſs wires inſerted into the tube, covered with a wet ſtuff like a fur of muſtard; and, another time, as I have related, in the num. 397 of *Artif. Elect.* I made the experiment with a drop of water ſupported between two iron points that met in the center of an almoſt ſolid ball of glaſs; when the latter, by the ſudden exploſion of the water, was ſhivered to atoms.

591. From the numerous experiments I have made on this ſubject, I conclude, I. That the diſtance between the metallic wires being given, the diſcharge meets with a greater reſiſtance againſt its paſſing united, and thus breaking the tube, when the latter is cloſed at both its ends and completely full, than it does either when the two openings of the tube are left open, or when being cloſed, there remain at both ends, intervals filled by air. II. When the tube is open, it reſiſts the more the ſpark in proportion as it is longer; that is to ſay, in proportion as the two cylinders of water, which the ſpark muſt drive off on each ſide of the tube, when it explodes the drop contained between the two wires, are longer.

592. In my Work on *Atmoſ. Terr. Elect.* I related experiments in which, by making uſe of very ſtrong ſparks, I broke tubes ſtronger than thoſe above mentioned, and drove their ſhivers farther (p. 75). Nay, I have made with ſealing-wax, a kind of electric gun, with which I can drive a leaden ball into a piece of clay, and make it ſtick to it. The fig. 2. pl. X. repreſents the ſection of an electric *mortar*. I. A H I K is a cylinder of wax cut at a right angle in A H; it has in E F an hemiſpheric chamber, which is deſtined to receive the wooden ball P; in D L is the ſmall chamber for the charge. II. I inſert into that cylinder a braſs wire B L, which reaches the bottom of the chamber deſtined

* According to an experiment made by Mr. Canton, a given body of water is not contracted by the whole weight of the atmoſphere, more than $\frac{1}{48,000,000}$.

to

to lodge the charge. III. I infert another wire topped with a ball in Q, the extremity D of which gets into the chamber DL. IV. With a pencil I place a drop of water in that chamber, and lodge the ball in the great chamber. V. I lay the mortar on the coated plate, and at the fame inftant that I approach the conducting bow in G, the fpark excited between the two wires BL, DC, makes the drop of water fly into vapour, and drives the ball P to the diftance of twenty feet. When I make ufe of three large charged jars, the ball is driven fo far as twenty-five feet diftance; if, to the drop of water, I fubftitute a drop of fpirit of wine, the diftance to which the ball reaches is ftill greater; and ftill more fo, if the fpirit of wine is impregnated with camphire. It appears, that the inflammability of liquors co-operates with the expanfion effected by the difcharge; yet, inflammable liquors being of an *infulating* nature, oppofe a refiftance to the paffage of the fire.

563. Thefe ludicrous experiments, neverthelefs, explain the moft admirable electrical phænomena that take place in nature: fuch undoubtedly are, I. The general light diffufed over houfes covered with rain-water, or over the furface of rivers, where a ftroke of lightning takes place. II. The violent explofion of liquids effected by lightning. III. The fhivering to pieces of the fhoes of thofe who are ftruck by lightning, which is the effect of the moifture of their feet flying into vapour, as alfo its rending wet clothes into rags. Nay, from thofe experiments I have drawn an explanation of one of the moft extraordinary phenomena in nature; that is, the fall of ftones from the fky. Pythagoras afferted, that they came from the fun, perhaps becaufe he faw them to be either red or black, as if they had been burnt. The circumftances of the fall of one of thefe ftones, accurately afcertained and tranfmitted to me by Monfignor Fogliani, bifhop of Modena, has convinced me, that thofe ftones are driven by lightning, exactly in the fame manner as the ball was flung from my electric mortar by the fpark; but of this I fhall treat at fome length in my book on *Nat. Elect.*

594. Propofing now more particularly to examine into the phenomenon of the running of fparks over the furface of water, to

 very

very great diſtances, while they open to themſelves but very ſhallow paſſages through the ſubſtance of the ſame, I think I muſt make a digreſſion on my former principle, that *the electric fire diſpoſes deferent bodies into its paſſage*; this digreſſion, though it may be ſomewhat long, is neceſſary, becauſe our preſent ſubject, and all our ſubſequent experiments depend on it.

595. This old principle of mine is applicable not only to a few electrical motions, but univerſally to all ſuch as proceed from a diffuſion of the electric fire; the oſcillation of electric pendulums, the ſuſpenſion of corpuſcles, &c. but I ſhall for the preſent only mention a few of the ſimpleſt facts. Rub ſoftly the glaſs cylinder of the electric apparatus, and then preſent to the conductor a baſon filled with bran, you ſhall ſee the ſtream of electric fire making as it were a long row, or tract, with the particles of the bran, driving and diſpoſing them into its paſſage.

596. In pag. 52 of *Artif. Elect.* I diſcharged the coated plate through a candle placed on the plate, and the diſcharge paſſed through the flame, lengthening it into the ſhape of a T, by expanding it towards the head of the conducting bow, and towards a ball of metal placed near it, and raiſed on the plate; that is to ſay, the diſcharge diſpoſed the flame of the candle into its paſſage, this flame being able to tranſmit it.

597. I have already related the experiment made by ſending a diſcharge to a greater or leſs diſtance, through one or more drops of mercury; the diſcharge diffuſes itſelf into thoſe drops, and drives them into vapour; part of this vapour, if attentively obſerved, is ſeen to riſe in the air under the appearance of ſmoke, and the other part remains imprinted on the glaſs.

598. A diſcharge will likewiſe melt the particles of hard metals, and drives into its paſſage the deferent vapours ariſing from them, and alſo ſuch particles as are not completely melted. Place on the margin of the naked plate HG (Pl. IV. fig. 9.) ſome filings of iron or of braſs, and ſend through them the ſtrong charge of the coated plate AB; you will find the paſſage of the fire marked by the ſmutty track which the effluvia from thoſe filings have left. Lightning has alſo ſupplied me with proofs of the truth of the above principle, as we ſhall hereafter ſee in its proper place.

599. Dr.

599. Dr. Prieſtley has thrown an additional light on the truth of this ſame principle, by ſeveral experiments of his own invention, which are contained in his ſixth ſection. He has, for inſtance, ſent diſcharges through interrupted tracks of braſs filings, and through ſuſpended drops of water; and always the braſs filings moved forwards to meet the next ones, and thus complete the intervals left between them; and the ſuſpended drops aſſumed a lengthened ſhape, ſtriving, as it were to join with each other (after the ſame manner as I repreſented, ſea trumbs, in my Work on *Nat. Elect.)* and the diſcharges were thereby enabled to run through ſpaces of two inches in length; whereas, in common air, they cannot leap to a diſtance greater than half an inch.

600. To this ſame principle muſt be aſcribed all the ingenious experiments made by Dr. Prieſtley in his XI. ſection, with reſpect to the ſending of diſcharges along the ſurfaces of bodies, and eſpecially of water; which is alſo the object that I propoſed here to treat. The reſult of Dr. Prieſtley's experiments (except ſome particular circumſtances, which I ſhall explain in their proper place) may be reduced to the following facts: he ſent diſcharges over the ſurface of water, of other liquids, of wetted ſolid bodies, over the ſurface of a touch-ſtone, and of a ſmooth lump of iron ore, to the great diſtances of ſeven, eight, or even eleven inches.

601. Now, I do not underſtand how Dr. Prieſtley happened not to perceive the dependence of all thoſe facts, on the principle expreſſed above; and that ſparks were enabled to run to ſuch great diſtances, along the ſurface of thoſe bodies, by the vapour which they themſelves excited as they paſſed. I imagine his caution againſt forming any conjectures of that kind, proceeded from his happening not to perceive any vapour ariſing from a very thin ſtratum of water, through which he ſent a diſcharge. Mercury, in winter time, is ſeen to ſmoke, and leaves marks behind it; common water leaves none, but will do it if it be coloured. With ſilk ſtrings I have ſuſpended to the arms of a balance, two thin ſmall plates of glaſs of equal weights, which ſerved as baſons to it; I wetted them both with a pencil, ſo that their weights remained equal to one another; and after ſending a diſcharge through one of them, I always found that

it had loſt ſome part of its weight, and the other preponderated. However, Dr. Prieſtley's experiments prove this, that a very ſmall quantity of moiſture is ſufficient to ſupply the quantity of vapour required to ſerve as a vehicle to the electric fire. The experiments are alſo very intereſting which he made with a piece of wood that had juſt been dried; though the wood could at firſt tranſmit a diſcharge, two hours after it could no longer do it; the ſame may be ſaid of the experiment he made with a bladder; both demonſtrate, that little moiſture may ſuffice to enable a ſpark to run through a *long* paſſage; but that, on the other hand, this little is neceſſary.

602. The obſervation made by the above ingenious Obſerver, that ſparks ran along the ſurface of a touch-ſtone, and alſo along that of a lump of iron ore, in the ſame manner as they did along the ſurface of water, has probably been the cauſe of the particular opinion he has adopted on the preſent ſubject. But if he had only examined the places along which thoſe ſparks had ran, he would have ſeen their crooked way marked by a continued track of a cinereous dark colour, which on the touch-ſtone inclined to reddiſh; that is to ſay, the diſcharge had in its paſſage exploded a certain quantity of metallic particles which abounded in the above ſubſtance, though they were too much parted from each other by terreſtrial and ſulphureous particles to allow a free paſſage to the ſpark; and the latter had been able to run to a great diſtance, only by its diſpoſing metallic effluvia from thoſe particles into its way, in the ſame manner as it would have done aqueous vapours, if it had been ſent over the ſurface of water.

603. That, from a touch-ſtone metallic vapours may be raiſed by a ſpark, is very probable, ſince its black colour is thought to be owing to its being replete with ferrugineous particles. But, beſides this conjecture, I have tried to aſcertain, whether ſparks can actually ſeparate the particles of ſuch ſtone, whatever they may be, and diſpoſe them into their paſſages. I filed into duſt a bit of touch-ſtone, and with this duſt I made a communication between the plate G H (Pl. IV. fig. 9.), and the coating of the plate A B; and I found, that the ſpark which I ſent through this track of duſt thus diſpoſed, had driven it away, and that there only remained, inſtead

inftead of the continuous track of duft, fmall interrupted heaps of it, the furface of which appeared of a cinerous colour. I blew them away, and then faw that the glafs indeed was not ftained in the very places which thofe heaps covered, but then the ufual ftain was confpicuous on all the intervals that had been left between thofe fmall heaps.

604. I make, with the fame duft, and at a convenient diftance from the plate GH, a fingle fmall heap of it; and as foon as I prefent the conducting bow to it, it is diffipated, and the ftain takes place over the whole fpace between the heap, and the edge of the plate. Generally thefe ftains fail in fome fmall interval around the top of the ball of the conducting bow; becaufe the fpark chufes in that place to go through the fhorteft paffage which is afforded to it by the convexity of the ball, and in that place only, leaps through the air.

605. From all the above obfervations it is evident, that an electric fpark, when fomewhat intenfe, drives not only water into vapour, but alfo raifes deferent vapours from ftones and iron ores, in its paffage over thofe bodies; and thus all the experiments of Dr. Prieftley contribute to evince the univerfality of the property of the electric fire, *of carrying into its paffage fuch particles as, in confequence of their deferency, or aptnefs to explode the refifting medium in any manner whatever, may facilitate its paffage.*

606. But, in order that the extenfivenefs of the principle may be adequate to that of the fubject, we muft alfo lay down the following rule, viz. *that a fpark fent over the furface of a body, always will diffufe itfelf into the fubftance of that body, in an inverted ratio to the refiftance it will experience from it.*

607. A fpark, befides the refiftance it meets from the fuperficial particles of the body which it feeks to explode, is alfo oppofed by the air: it even muft exert itfelf the more againft the latter's refiftance, in proportion as it can lefs penetrate into the fubftance of the body over whofe furface it runs.

608. Therefore, I. If the body be of itfelf perfectly deferent, the difcharge will wholly penetrate through it, and it will be impoffible to conduct it along the furface; hence it is no wonder that Dr. Prieftley never was able to make a difcharge run along the furface either of mercury, or melted lead.

609. II.

609. II. But if the inner ſubſtance of the body oppoſes a great reſiſtance to the ſpark, and, at the ſame time, if the ſuperficial particles of that body are capable of yielding to the mechanic action, or to the penetrating heat of this ſpark, then the latter being repercuſſed by ſuch inward reſiſtance, will run along the ſurface, exploding on the one hand the reſiſting air, and on the other hand, penetrating into the ſuperficial pores of the body. And according as the particles of that body will be more eaſily driven into vapour or duſt, and, when in that ſtate, will be more deferent, the ſpark will run to a diſtance proportionally greater than it would be able to do, if it had to open a paſſage to itſelf through unmixed air, without the aſſiſtance of any vehicle.

610. From thence we may explain a variety of facts: a diſcharge finds much difficulty in penetrating to a great depth, through the ſubſtance of water, as is evident from the above experiment of the ſmall tubes; but, on the other hand, it can raiſe a very expanſive vapour from the ſurface of the ſame; it can plentifully diſpoſe ſuch vapour into its paſſage, and make it ſerve as an agent to drive the reſiſting air; the conſequence of which is, that it can run over the ſurface of water to the uncommon diſtance of ſix or ſeven inches.

611. A diſcharge ſent along the ſurface of ſoft dough, depreſſes it uniformly all along its paſſage; becauſe the ſpark is, in this caſe, repercuſſed by the air againſt the ſoft dough, with a force equal to that with which it itſelf drives this air, either by its own immediate action, or by that of the vapour which it raiſes.

612. This may ſerve to rectify the explanation I gave at pag. 120 of *Artif. Elect.* with reſpect to the ſudden concuſſion which a man feels in his hand dipped in water, at the inſtant that a ſtrong diſcharge is ſent along the latter's ſurface. The quick ſtroke which the ſpark, from the repercuſſion of both the air and the raiſed vapours, gives on the water, is communicated to the hand through the continued rigid parts of the water, and the hand receives in ſuch caſe the ſame kind of ſhock as a ſhip does in an earthquake, as I have obſerved in *Atmoſ. Terr. Elect.* The above mentioned commotion is, however, different from an electrical ſhock. I placed my hand, as Dr. Prieſtley had done before,

fore, under water, in the middle between two metallic rods, which tranfmitted a difcharge through that water; and when this difcharge took place, I felt a kind of convulfion in the mufcles of my hand; which convulfions were ftronger according as I took care to ftretch both my thumb and fmall finger towards the two rods; becaufe, in this cafe, the difcharge paffed more united to them from thofe rods.

613. A difcharge fent along the furface of a piece of ice, leaves on it fmall unequal cavities, which exhibit the fame appearance as if a fmall chain had been placed hot upon it. The hard furface of the ice does not yield to the mechanic action of the fpark; or, if it does, it can only do it by being broke in different places: befides, if a difcharge can melt the outer furface of cryftal (we fhall fee hereafter that it even makes diamond powder evaporate) it muft neceffarily melt ice, and the cavities obferved on the latter, accordingly exhibit all the ufual figns of inftantaneous liquefaction.

614. A difcharge fent along the furface of fnow, diffipates a great quantity of it, making a furrow in it two inches deep, and nearly as broad; but it does not leap to a greater diftance than three inches. A fpark fent through a body of fnow penetrates into it to a great depth, as its particles are much disjointed and moveable (in fact, a fpark fent perpendicularly into fnow, penetrates into a deeper ftratum of it than it would do into water), and thence proceeds that great diffipation, juft now obferved, of deep particles of fnow, when a difcharge is fent along its furface. To this circumftance is likewife owing, that the fpark can only run to a diftance of three inches over the furface of fnow, becaufe the fmall quantity of fire which keeps over this furface, has but little power to raife vapours from it. If you fend a difcharge through a long track of fweepings or duft from a brafier's fhop, the fpark will not pafs united to any great diftance, and will make a furrow along the top of this track; the reafon is, that the feparations between the deferent particles, and the duft and air that lie between the fame, will prevent the fpark from penetrating deeply enough into the track; and where it will do it, it will be obliged to drive and fcatter away fuch air and duft, and with thefe alfo the deferent particles of the filings.

615. A

615. A difcharge fent through a green leaf, tears its furface according to its own direction, and alfo perpendicularly to this fame direction, though, through fhorter tracts. The difcharge penetrates into the internal moifture of the leaves; and, by driving it into vapours, tears, in every direction, the exterior fibres; an image in miniature, this, of the tearing of large trees to pieces by the violence of lightning.

616. A difcharge paffes to a certain diftance over the furface of fpirit of wine, without inflaming it; but if it runs to a greater diftance over that furface, it fires it. This experiment fuggefts, it feems, feveral luminous ideas. Firft, it appears, that the facility with which the electric fire runs over the furface of moift fubftances, is proportioned to the facility with which thofe fubftances may be turned into vapour; to this principle, I think muft be attributed the effect of the ftrong fpark mentioned in p. 249 of *Atmof. Terr. Elect.* which from the hook of the bottle leaped and ftruck me at the diftance of four inches. And though a fpark cannot run along the furface of fpirit of wine to any great diftance without firing it, yet it moft eafily moves along it. I underftand very well how a fpark cannot penetrate into fpirit of wine; this is owing to the infulating nature of the latter; but that it fo eafily moves along its furface is what I cannot fo well explain; I attribute it to the facility with which the fpark can raife vapours from the fpirit of wine, that powerfully drive the refifting air; which explanation is alfo conformable to what has been above obferved with regard to the electric mortar, which could drive balls to a greater diftance, when loaded with a drop of fpirit of wine, than when it was with water.

617. Dr. Prieftley alfo fent difcharges along the furface of animal fluids, and obferved, that they produced a louder crack than thofe on the furface of water, efpecially on the furface of milk, and whites and yolks of eggs; and I have obferved that when, in order to increafe a difcharge, I make ufe of fpittle inftead of water, a more united and louder crack takes place; this I attributed to the greater *volatility* of animal fluids: if I ufe a drop of lemon juice, the fpark paffes more difficultly, and produces a lefs crack. This fame principle (the difference in the volatility of fubftances) will, I think,

I think, alſo reſolve the queſtion why diſcharges refuſe to paſs along the ſurface of recent glaſſes, though they are in ſome degree deferent, and conſequently ought, it ſeems, to afford a paſſage to the fire; why it does not run along the ſurface of alum, of rock-ſalt, of blue vitriol, of red vitriol, of poliſhed agate, all of which bodies are in ſome degree deferent; why it does not move along the ſurface of dry wood, of dry paſteboard, or of the covering of a book, however poliſhed.

618. The circumſtance of the ſurface being poliſhed is of little importance with regard to the running of the ſpark over it; it moves freely enough along the ſurface of a touch-ſtone, however rough it may be, and on wrinkled fleſh, bending its courſe, in conſequence of the repercuſſion of the air, around all the angles and prominences it meets in its way: the volatility of the particles of theſe ſurfaces, joined with the nature of the bodies themſelves, which refuſe to admit internally any great quantity of electric fire, are the cauſes of the above effects. Neutral ſalts are of a very fixed nature, if compared to water; agate and glaſs have no particles that fire may diſtend to any great degree; and wood as well as paſteboard, if you take all moiſture from them, will be found to be of much the ſame nature as the above bodies.

619. If a quire of paper has been dried before the fire, a ſpark will paſs through it with more difficulty than formerly; it will paſs more divided, and bore a leſs hole. If you wet it in the place where you intend to preſent the conducting bow, the ſpark will make a hole much larger, emit a much more vivid light, and produce a much louder crack; now, ſparks will run along the ſurface of bodies to a certain diſtance, in the ſame manner, and in the ſame circumſtances, in which they paſs through a moiſt quire of paper, or any other ſubſtance which may be eaſily and greatly dilated by the action of fire.

620. Dr. Prieſtley ſent a diſcharge over the ſurface of a leg of a ſheep juſt killed, and it ran along it to eleven inches diſtance; over the *medulla ſpinalis* of an ox, it ran to ten inches diſtance; does not the reaſon of theſe two ſurpriſing effects lie in the greater alcaleſcence, and conſequently the greater volatility, that take place in the fluids of dead animals?

On the action of ſparks on living bodies.

621. The above obſervations on the effects of electric ſparks with regard to liquids, naturally lead us to examine their effects with regard to living bodies; the following previous obſervations muſt, however, be made, viz. that dried fibres, either of animals or plants, the fat ſubſtance of animals, and the oily, or the reſinous juices of plants, are all of them of an inſulating nature. After a long drought, I have found a muſcle, and a wooden table, capable of inſulating very well, and uſed them for that purpoſe, without any farther apparatus; with regard to oils, every one knows, that by excluding moiſture from bodies, they render them inſulating; and with regard to roſins, their electrical uſes are ſo generally known, that to mention any particulars about them, would ſurely be needleſs.

622. It, therefore, appears, that the electric fire *does not move through living animals, but inaſmuch as they are impregnated with fluids which participate of the nature of water; nor does it ſhake, diſtend, burn, and conſume the ſolid parts of their bodies, but inaſmuch as it runs through ſuch fluids and explodes them, or guſhes out of them againſt the ſaid ſolid parts.* We ſhall ſee hereafter, that it is one of the characteriſtic effects of lightning, to dry and ſtiffen the living bodies it ſtrikes, and to ſplit trees in the direction of their fibres, which is alſo the direction in which they draw their nutritive juices; we ſhall alſo ſee, that it ſplits more particularly thoſe fibres, which more copiouſly draw ſuch nutritive juices, and that it even ſeems to chooſe out thoſe trees among others, whoſe juices are of a more aqueous nature, or which, *ceteris paribus*, are more replete with watery particles.

623. But attending here only to ſuch obſervations as Art can ſupply, I ſhall diſtribute the matter of this chapter into four principal parts: in the firſt I ſhall ſpeak of the effect of ſparks on living bodies; in the ſecond place, I ſhall treat of the effects of electricity, when it is exerted without ſparkling; in the third place, I ſhall offer ſome conſiderations on the uſe which phyſicians may make of electricity; and laſtly, I ſhall ſpeak of the uſe which Nature herſelf makes of it.

I SECT.

SECT. I. *Of the action of ſparkling electricity on living bodies.*

624. *A ſpark runs through that ſeries of muſcles, where the reſult both of the electric capacity, and of the ſhortneſs of the paſſage, is moſt favourable to it.* Let the man A graſp a bottle with his right hand, and hold a braſs rod with the left; let him touch with his rod the naked right foot of a man B; let the ſame man's left foot communicate through a braſs rod with the right foot of another man C; let the ſame man C, through its left ear, communicate with the right hand of the man D; laſtly, let the man D with his left hand touch the hook of the bottle. The man A, whoſe electric communication is formed by his arms and hands, will be ſtruck through that ſeries of muſcles, which is moſt continued from one of his hands to the other; that is, he will be ſtruck in the muſcles of the right hand, of the right arm, of the thorax, of the left arm, and of the left hand. The man B will be ſtruck in the muſcles of his right foot, his right leg, and right thigh, and in thoſe which are connected with his left thigh, leg, and foot. The man C will feel the ſhock in that ſeries of muſcles, which from his leg, through which he communicates with the man B, proceeds to his ear, through which he communicates with the man D, &c.

625. In the above inſtances, the ſpark, I. *exerciſes a ſimilar action in ſimilar parts of the body, which are ſimilarly ſituated, and through which it runs with a ſimilar denſity.* II. *It extends its action to ampler parts of the body, according as it conſiſts of a more copious and denſe fire.* III. *It extends alſo its action to a greater diſtance around the ſaid parts, according as its copiouſneſs and denſity are ſtill more increaſed.* IV. *And its action is moſt intenſe, where, from the reſiſtance it meets, and the narrowneſs of the paſſage through which it is led to paſs, it is brought to the greateſt denſity.*

626. I diſcharge a glaſs, by applying the index of my right hand to one of its coatings, and the index of my left hand to the other coating; the ſpark ſtrikes both my fingers in the ſame manner; it alſo ſtrikes in the ſame manner my two wriſts, my two elbows, my two ſhoulders. However, in order that the

ſpark may ſtrike all of them equally, I muſt place them in ſimilar ſituations, becauſe a muſcle in a ſtate of contraction will always be ſtruck in another manner than a muſcle in a ſtate of relaxation: it is needleſs to add, that the ſpark muſt leap in a ſtate of equal denſity to produce equal ſhocks; if, for inſtance, a finger is preſented to the coating of a plate, and another to a ſharp point annexed to the other coating, the latter finger will receive a more ſenſible ſtroke than the former.

627. That the ſpark extends its action to more numerous parts of the body, according as it conſiſts of a more copious and denſe fire, is evident. If with a finger of each of my hands I draw from a plate a very ſmall remnant of charge, I only feel a ſmall ſhock in thoſe fingers; if the remnant be ſomewhat greater, my fingers are contracted by the ſtroke; if the remnant be ſtill greater, I feel the ſtroke in my wriſts and elbows; if I draw the intire charge of a jar, I receive a ſhock throughout all the ample muſcles of my ſhoulders and thorax.

628. The ſame holds true, if the ſpark enters and iſſues through diſſimilar parts. The ſpark which I excite from the conductor, while I ſtand on the ground, gives a ſtroke only to the muſcles of my finger, becauſe the ſpark while it flows through the ample muſcles of my legs, or the ample *ſurface* of the ſoles of my feet, has no ſufficient denſity in theſe parts to produce any ſenſible ſtroke; but if I keep one of my feet at a diſtance from the ground, I begin to feel a kind of ſtroke in the muſcles of my other leg, which remains placed on it; and if, being inſulated, I preſent one of my legs to a ſtrange body, the muſcles of the ſame are contracted correſpondently to the place where the ſpark is made to condenſate itſelf and iſſue; and I feel a proportional ſtroke.

629. Experience will alſo make the third point evident, viz. that the ſpark extends its ſhock more amply around the part through which it paſſes, according as it conſiſts of a more copious and denſe fire. A middling diſcharge gives me a ſhock in my thorax, and in a pretty narrow ſpace between my arms, through both which the fire paſſes: a more intenſe diſcharge ſtrikes me in a greater compaſs, the ſhock extends to the muſcles of my back, of my belly; and when Dr. Franklin inadvertently ſent through both his

his arms the difcharge of two large cryftal jars, he felt, as himfelf relates (Franklin's Works, pag. 153.) a general fhock from his head down to his feet: the reafon of thefe effects is, that the *wave* of the electric fluid, however rapid, muft in propagating itfelf through the fluids in the body, drive before it the fire which is naturally diffufed within them, which fire is compelled to flow to fome lateral part, proportionally to the amplenefs and denfity of fuch wave (Newton Princ. Prop. XLII.)

630. That the fpark in its paffage through the body of a man produces a greater effect in thofe places where it is fome how brought to a greater denfity, is evident from our own feeling, and is a neceffary confequence of the univerfal principle according to which the electric fire operates. We particularly feel the effects of the paffage of the fire in thofe parts where the fpark enters, or from which it iffues more condenfated; we feel a particular contraction in the mufcles of our fingers, of our wrift, and of our elbow, where the fpark from the body of one mufcle gets into that of another, through the teguments that are formed around them by the adipofe membrane, &c.

631. I think I make fuppofitions conformable to truth, when I fuppofe the paffage of the fpark to be through the mufcular fyftem. Whether it be that the mufcular fluid of itfelf admits the fpark the moft readily, or whether, by having remained longer in the body of the animal, and being more braced and difpofed to evaporation, it is rendered more deferent, I do not pretend to fay; but it evidently appears from the contraction of the mufcles, fuddenly effected by the paffage of a fpark, that this paffage takes place through them. Obferve with attention, when you draw a fpark from the hand, the arm, or the leg of a man, and you always will fee that particular kind of motion take place in a mufcle, which it is of itfelf inclined to perform. We, indeed, meet with inftances in which fparks, and more fo the enormous ftream of a ftroke of lightning, is partly diffufed into fome fanguineous veffels; we even fometimes fee marks of clotted blood in the fmaller fuperficial veffels of pretty large animals, through the bodies of which ftrong difcharges have been fent; but this does not contradict what we faid above, that a difcharge prefers to run through the mufcular fyftem.

632. To

632. To that end, I have more attentively examined the contractions which fparks produce in the mufclesof animals, by repeating on the mufcles of the thigh of a ftrong cock, the experiments which I formerly defcribed in p. 129 of *Art. Elect.* and Signor Buffone, furgeon in the hofpital of S. Giovanni, has been fo kind as to affift me. We faftened the cock to a chair, its wings were tied to the higheft crofs-ftick of the back of the chair; its right leg was faftened to the loweft ftick, a man held its head, and its left leg was left at liberty, for the furgeon to operate on it. The thigh being ftripped of all its common teguments, without lacerating its flefhy fibres, I began to fend fparks through the feveral mufcles which remained united, and in their natural pofition, fucceffively. I ufed my ufual jars, which contain at moft half a pint; with one hand I held the belly of it, to which was faftened a brafs chain; with my other hand, and a ftick of fealing-wax, I managed the other end of the brafs chain; I prefented this end of the brafs chain to the extremity of a mufcle, and the hook of the bottle to the other extremity; and the paffage of the fpark being thus evidently determined, I conftantly made the following obfervations, I. Any one of the mufcles, when traverfed by the fpark, was vehemently contracted, fo that it always compelled the hand of the furgeon to give way. II. The contraction of the mufcle always was attended with a fudden and proportionally violent fwelling of it; meanwhile, the places where the membrane which parts one mufcle from another, was inferted between them, remained depreffed. III. In the place through which the fpark ran, the membrane which vefted the mufcle, from moift and fmooth became in an inftant dry and wrinkled, the direction of the wrinkles being tranfverfal to the length of the mufcle, and a very confpicuous vapour, or fmoke, arofe over that place. IV. The drynefs, the wrinkles, and the fmoke, continued after the fpark had paffed; but thefe different accidents afterwards gradually vanifhed, fo that the membrane partly recovered that kind of glofs, which is the refult of moifture; another fpark being fent afrefh, the fame appearances again took place. V. While one mufcle was thus contracted, a kind of general contraction was obfervable in all the mufcles contiguous to it; and there was hardly

hardly a mufcle in the thigh that remained in a quiet ftate, which I have confidered as a vifible proof of the expanfion of the fhock into lateral parts. VI. For feveral feconds after the paffage of the fpark, fmall convulfions took place in the mufcle, and it was during that time that the above mentioned wrinkles began to leffen, and a new moifture gradually appeared on the mufcle.

633. I afterwards got the body of the mufcle which ftretches itfelf along the lateral furface of the thigh, and which till then had been let alone, to be feparated from the mufcles placed under it; and, befides the circumftances obferved above, all of which took place in this mufcle, I made the following experiment. We parted the body of the mufcle from the thigh, by paffing our finger under it and drawing it out, and at the fame time holding firm the articulation of the leg, fo that the mufcle could not return to its place; which done, the body of the mufcle appeared relaxed, and remained parted from the thigh; but, at the inftant that the fpark was fent, the mufcle violently contracted itfelf, and was drawn back into its natural place; nor could it be difplaced again but by being afrefh forcibly drawn out by the fingers: when thus parted again, a new fpark again replaced it in its former fituation. If one of the fides of the mufcle being twifted out, the fpark was fent only through the *venter* of the fame, the fide did not exactly recover its former fituation; but if a fingle new fpark was fent along the fide, the mufcle inftantly recovered its natural fituation.

634. The furgeon, before he parted this mufcle from the others, pricked with his fcalpel the mufcle next to it, pretty deeply, but for all that no great contraction enfued. Having tortured the parted mufcle with a number of fparks, the furgeon cut with fciffars fome lateral fibres, and the mufcle fcarcely moved; it feemed as if the preceding fparks had rendered it infenfible to the ftimulus of the inftruments, and frefh fparks alone could contract it: I fent fome difcharges from the bottle into a wound made with the fcalpel; the fame contraction took place, and the edges of the wound remained particularly turgid and livid, with fome fmall clots of blood around them.

635. I

635. I afterwards fent in the dark feveral fparks through different mufcles, and I could not fee them lighten, either at their entering it, or at their iffuing from it; I, therefore, concluded, that they penetrated and went through it.

636. From the whole of thefe obfervations, I think that feveral interefting facts are either difcovered or afcertained. Firft, the fudden aridity, and the fmoke, which are produced by the fpark in the place of its paffage, manifeft the explofion of the moifture or fluid, with which the mufcle is impregnated; which fluid is, of itfelf, very apt to evaporate and diffipate, as may be obferved in animals frefh killed; and the bare rugofity of the membrane is a proof of the diffipation of this moifture or fluid: and is not the contraction of the mufcle, fo fuperior to that caufed by any incifion of its fibres, a proof of the fudden and violent expanfion of the faid moifture, or fluid, effected by the denfe, penetrating, and heating igneous element, which proportionally diftends and fwells the fibres of this mufcle.

637. From thefe violent contraction of mufcles, a vaft field is here opened for conjectures about the caufe of the voluntary contractions of mufcles; but fuch inquiries are too foreign to our fubject to engage in them at prefent; I will, therefore, content myfelf with laying down the following maxim, viz. *that thofe violent contractions above defcribed, are of the fame kind with thofe produced by the action of the electric fire when it kills a moufe, a bird, a pigeon, a chicken, or even a man, in confequence of its rufhing through them with a copious proportional ftream*; the fire in thofe inftances fo much diftends and fwells the fluid of the mufcular fyftem, that it injures the whole fyftem itfelf, and renders it unfit for performing again its vital functions. I even think that fuch a ftream can kill, merely by caufing a damage to the body of the fame kind as might arife from a prodigious and fudden *wearinefs (ftracchezza)*.

638. I. The copioufnefs of the ftream muft be proportioned to the amplenefs of the fyftem through which it is to run. With a fingle fpark from a coated fheet, I can kill a moufe, a fparrow, &c. To kill a pigeon, one fpark is not fufficient; three are requifite, or even more, that fucceed each other pretty quickly, and

and before the vital force can repair the injury occafioned by the preceding fparks; I even think, that a man might be killed with multiplied fparks from fimple fheets, if they were fent quickly one after the other.

639. II. The particular kind of effect caufed in animals, when they receive a killing ftroke from a ftrong fpark, is a beginning of a violent contraction, but which does not proceed farther, becaufe the univerfal contraction which takes place in every antagonift mufcle, prevents it. Soon after this beginning of contraction, a more or lefs violent diftenfion takes place in all parts of the body; when the ftream is very copious, the animal dies in this beginning contraction, and the enfuing diftenfion does not obtain; thus we have inftances of men ftruck by lightning, who have remained dead and ftiff in the very pofition they were in when ftruck.

640. III. When an animal is ftruck dead by a fpark juft fufficient to kill it, it appears, that it does not die either in confequence of any rupture of its fibres, or any extravafation of its liquids. In various animals killed by a fpark, I never could, though affifted by expert perfons (the fame has happened with Dr. Prieftley) perceive any rupture in the thorax, or any wound that penetrated under the fkull into the fubftance of the brain. In a pigeon killed by a fpark, there was indeed a reddifh fpot in the pericranium, but more internally nothing could be perceived; and there are, on the other hand, inftances of men killed by lighning, on whofe bodies no vifible figns of wounds, or in general, of the leaft injury were to be perceived; hence it feems, that even when lightning enters fo divided, as to leave no marks of injury on the furface of the body, yet, if it enters with fuch a copioufnefs that it may contract to a great degree the mufcular fyftem, it kills the animal.

641. IV. To this muft be added, that with fome proper affiftance I have reftored to life animals that had been ftruck with fparks juft fufficient to kill them. I once fent the difcharge of two large cryftal jars through the head of a hen; the hen, after fucceffively dropping down its head, and ftretching her legs, in lefs than a minute lay completely motionlefs, and rofe no more.

 I ftruck

I ſtruck in the ſame manner another hen, rather inferior in ſize, or at moſt equal, to the former; ſhe fell as the former had done, but after fifteen or twenty ſeconds had elapſed, we began to rub her all over her body; when ſhe ſoon began to raiſe again her head, contract her legs, and looked around herſelf, as if ſhe ſought for ſomething to lean upon, but could not riſe; having opened her beak, we poured drops of ſpirit of wine into her throat, and ſhe ſoon roſe and lived. Hence it is evident, that electric ſparks, or lightning, can kill without any material rupture of organical parts.

642. V. Death may alſo follow a ſtroke from a ſpark, or from lightning, though no ſuch quantity of the vital fluid has been diſſipated, as may create a mortal *ſiccity* in the body. Having ſtrongly charged the coated plate B A, I diſcharged it through a mouſe, which was tied to a piece of glaſs: the mouſe was killed; and, as I had weighed it immediately before the diſcharge (it weighed 7 den. 13 $\frac{1}{2}$ grains) I found after it, that it had only loſt about the third part of a grain. Now, I do not think that ſuch a ſmall quantity of animal moiſture was neceſſary to the life of the mouſe; but, on the other hand, if we conſider both the enormous dilatation which this ſmall quantity of fluid may have ſuffered when exploded by the ſpark (I think it may have been dilated to a volume fourteen thouſand times greater than it was before) and the rapidity of the ſtroke of this ſpark, we ſhall not be ſurpriſed, that a mortal *wearineſs* enſued. In this mouſe I could not perceive the leaſt ſign of any, either internal or external, ſolution of continuity.

643. It really ſeems, that a certain degree of that kind of injury, which I expreſs by the word *wearineſs (ſtracchezza)*, is more to be dreaded than any wound, fracture, or burning, from lightning. Four of the eight children who were ſtruck by lightning under the tree of *Moniſtero*, in the neighbourhood of Mondovi, (*Atmoſ. Terr. Elect.* p. 240.) had their ſkin ſtripped off in different parts; the one, all over one of the cheeks, the other off both the ſhoulders and a little below his back bone, the third near the back of the neck, where the hair was burnt and his hat bored through, and the fourth, along the arm; but none of theſe wounds were deep,

nor

nor broader than one inch : they all were feemingly dead, deprived of fenfe and motion ; but, when carried home, recovered in lefs than one hour, except him who had been wounded on his head ; who, after feveral hours, continued to be looked upon as dead ; but his mother having applied her hand to his breaft, felt fome pulfations of his heart, and upon his being bled, he alfo recovered. Three days after, I went to the fpot to fee them ; when, being invited by the promife of fome fmall gratification, they came to the Judge of the place ; but both their look and carriage, indicated a degree of *wearinefs*, even greater than that of the harveft-men in the *Campagna Romana*.

644. VII. Even the men ftruck in the dungeon of the citadel of this place (*Turin*), even many of the perfons who were ftruck by that tremendous ftroke of lightning at *Villa di Stellone*, have recovered. I think that when the electric fire ftrikes, united enough, fome particular place of the human body, fo as to tear off the fkin, the vapour which it then raifes, can in a great meafure conduct the ftream along the furface of the body, and thus leffen the internal deadly fhock. It feemed to me, that I plainly faw fuch a deviation of the electric ftream take place on a poplar that had been ftruck by lightning in the plain of Mondovi : its branches, from the middle to the top, had been dried by the effect of the ftroke ; with this circumftance, however, that the upper ones remained erect ; the lower ones, which were very long, were partly broken, partly bent downwards, and directed towards a rivulet which ran clofe to the foot of the tree. It is probable, that the ftrength of the flafh had difpofed thofe boughs into its way, and the moifture that was forced out of them (I was told, that the ftroke of lightning preceded the rain) had been fufficient to conduct the fire out of the tree through the air, and thus caufed the lower part of it to be fpared. But the ftrong fparks before mentioned, which run with fuch copioufnefs and light over the leg of a fheep frefh killed, and the medulla fpinalis of an ox, are ftill more evident proofs of the propenfity of the electric fluid to flow along the outfide of the body of a man, when it has once torn off the fkin, and fcattered a deferent moifture over the body.

645. VIII.

645. VIII. This hypothefis, that lightning ufually kills by its caufing a fudden violent *wearinefs* in the mufcular fyftem, is alfo confirmed by obferving that the flefh of chicken killed by an electric fpark, eats more tender than ufual; the bodies of perfons killed by lightning pafs fooner to putrefaction; the prodigious contraction of all the fibres in the body of chicken, muft render them tender in the fame manner as driving and hunting foften the fibres of an ox, or a ftag; and thence we may conclude, that the denfe penetrating deadly fire, may alfo raife the liquids in the body of an animal to a great degree of alkalefcence.

Sect. II. *On the action of electricity on living bodies.*

646. Though the profeffed fubject of this chapter was to treat of electric fparks, yet, I hope the reader will excufe the following digreffion about electricity without fparks; its effects are too much connected with the matter of the preceding article, to refer the difcuffion of them to another place.

647. Firft, the Abbé Nollet has found, by accurate experiments, that the electricity without fparks, promotes the evaporation of liquids proportionally, not fo much to their furface, as to their natural propenfity to evaporate: the real fact is this, *the evaporation is not, and cannot be, proportioned to the abfolute furface* (ceteris partibus) *of liquids, but muft be, and is, proportioned to the* liberty *of the furfaces themfelves.* By the words *liberty* of the furfaces, I mean their not being counteracted by an electricity homologous to that by which they are themfelves animated.

648. I procured fmall canns of tin, two inches high, cylindric, and eight lines wide; I filled the one with water, and placed it at the bottom of the electric well; the other I left in the open air, and put into it only half an inch of water, and placed it fix feet diftant from the firft conductor; I placed next to it a third cann intirely filled with water; afterwards I infulated both the canns and the electric well, and made them communicate with the conductor, through iron wires that touched their bottoms. Now, neither the cann placed in the bottom of the well, nor that

in

in which the furface of the water was eighteen lines lower than the brim of it, had, in three hours time, loft any thing of their weight, but the cann which was quite full had loft a grain, and even fomething more of its weight.

649. I. I fufpended to the conductor one of the fame canns full of water, at the diftance of three inches from the inferior furface of it; and to the bottom of this cann, I faftened a plate of tin, one foot wide. II. I fixed another cann on the conductor itfelf, which was alfo full. III. I infulated two other canns, at fix feet diftance from the conductor; the one was on the one fide of the conductor, and the other on the other fide; and over the one I fufpended, at five inches diftance from its furface, a circular plate of tin one foot in diameter. The electricity having been continually excited for three hours together, I found that the cann fufpended to the conductor, had loft nothing of its weight; that which was placed upon it had loft about half a grain; that which had nothing fufpended above it, had loft about a grain; and the laft, above which a plate of tin was fufpended, had loft a grain and an half of its weight.

650. Hence we fee, that the evaporation was nothing where the furface of the water was counteracted on all parts by an homologous electricity: in the cann placed on the conductor, only a little evaporation had taken place, becaufe the atmofphere of the conductor bent itfelf over the furface of the water in that cann, and leffened the evaporation; in the cann placed at a great diftance from the conductor, the evaporation was great; but it was ftill greater in the cann over which the plate of tin had been fufpended, becaufe a contrary electricity was actuated in that plate by the atmofphere of the cann: in confequence of this, a continual diffipation of vapour was effected by invifible effluvia of the electric fire, which, through the plate were enabled to be diffufed away: from this fame fact, we may alfo derive a new confirmation of the principle above mentioned, that the electric fire carries and difpofes deferent bodies in its way, &c.

651. Thence we fee, that *the evaporation of liquids is not always proportioned to the amplenefs of their furface*; becaufe, as the electric atmofpheres of liquids are much lefs counteracted by the

the reaction of the atmofpheres of the adjacent parts, clofe to the fides of the veffels in which they are contained, than in other places, their evaporation muft, of courfe, be proportionally greater.

652. A number of other circumftances may alfo alter the quantity of electrical evaporation. I. A fquare veffel will, as I have experienced, produce a greater evaporation than a circular one, becaufe the electric fire more eafily diffipates through the angles of fuch veffel. II. The liquor in a given veffel will evaporate the fafter, as the height of its edge above the furface of the water, will be in a fmaller ratio to the width of it; this is becaufe the electricity of the edge will then counteract the lefs the electricity which promotes the evaporation of the fluid. With regard to other circumftances, fuch as the temperature of the air, the drynefs, or dampnefs of the weather, &c. I think it would be needlefs here to expatiate upon them.

653. Mr. Boze, a great admirer of electric phenomena, once communicated the following obfervation of his own to the Abbé Nollet: *Electricity* (he found) *makes water run under the fhape of a continued brufh, from fuch fyphons as, on account of the fmallnefs of their orifices, would only emit it in much interrupted drops.* The Abbé, as a confirmation of this experiment, found afterwards, I. That water may not only be made to fpring out of the above mentioned fyphons, under the fhape of a continued brufh; but that the quantity of the water that iffued thus, might alfo be much increafed. II. That the quantity of water fpringing from fuch a fyphon, increafed the more (relatively to what it would have done if left to itfelf) in proportion as the aperture of the fyphon was lefs. III. That electricity can make water fpring from fuch fyphons as have too fmall orifices for any water to iffue out of them, even in drops. IV. That when the orifice of the fyphon is fo wide as to let water flow out of it in a continued ftream, then electricity accelerates it but little. V. When the orifice of the fyphon is a line wide, electricity neither accelerates nor retards the water. VI. And, laftly, the Abbé concludes with obferving, that when the orifice is more than a line wide, electricity rather retards than accelerates it.

654. From

654. From theſe experiments, the Abbé was led to conſider organized bodies in general, as aſſemblages of capillar tubes, filled with a fluid which endeavours to circulate through them, and ſometimes to ſpring out of them; he, therefore, electrified fruits, green plants, wetted ſpunges, and found, that they loſt more of their weight than ſimilar bodies not electrified.

655. Mr. Maimbray, in Edinburgh, electrified two mirtles, during the whole month of October, in the year 1746, and obſerved, that they vegetated more quickly than other mirtles not electrified. Encouraged by this diſcovery, the Abbé Nollet made farther experiments, and found, that ſeeds riſe ſooner in electrified veſſels, than in veſſels which are not. About that ſame time, Mr. Jallabert, Mr. Boze, and the Abbé Menon, made experiments of the ſame kind: and, firſt, Mr. Jallabert obſerved, that electricity, while it promotes vegetation, promotes likewiſe evaporation; which he eaſily aſcertained by weighing the decanters filled with water, on which he had placed onions of various flowers, and by comparing afterwards the remaining weights of the electrified decanters, with the weights of thoſe which had not been electrified.

656. From the vegetation of organic bodies, the Abbé paſſed to experiments on animals, on cats, pigeons, bullfinches, chaff-inches, &c. and he likewiſe found, that electricity always promoted their inſenſible tranſpiration; for, thoſe animals who had been electrified, were always found to have loſt more of their weight than others had done. Nor did the Abbé neglect to experiment on men; he found, I. That a perſon of a good age and health, by being electrified during five hours, loſt ſeveral ounces more of his weight, than uſual. II. That the perſon did not receive thence the leaſt inconvenience, only a kind of weakneſs, and a better appetite.

657. The Abbé concluded his experiments, with obſerving, that not only ſuch animals as are electrified, but even thoſe that ſtand near them, tranſpire more than uſual, and thence he endeavours to confirm his hypotheſis of an *affluent ſubſtance*; but his valuable experiments had no occaſion to be thus connected with an ill-grounded theory.

SECT. III. *On the medical uſes of the ſparkling, and not ſparkling electricity.*

658. Whoever has a mind to ſee how many attempts have been made to render electricity uſeful towards reſtoring, or preſerving, the health of man, may read the copious collection of experiments for that purpoſe, given by Dr. Prieſtley in his valuable Hiſtory of Electricity; as to me, if I here offer to ſay any thing on that ſubject, which belongs to a particular *Faculty*, it will be rather with an intent to aſk the opinions of other perſons, than to offer mine.

659. And, firſt, with regard to that which belongs to the *ſparkling* electricity, does it not evidently appear from what has been above ſaid, concerning thoſe who die from a ſtroke of lightning, that many of them might have been effectually aſſiſted? Now, which method of aſſiſtance would have been the beſt in ſuch caſe?

660. II. Is the idea of introducing medical effluvia through the ſkin of man, by the means of electricity, repugnant to nature, either with regard to the effluvia themſeves, or to the particular method of introducing them? I am aware, that I can hardly diſcuſs again this queſtion, which is commonly looked upon as having been fully confuted, without expoſing myſelf to ridicule; but why ſhould the laughter of unreflecting men prevent reſearches, the object of which is to promote the public good. Beſides, I do not propoſe here to revive the old queſtion on this ſubject, which I confeſs I always looked upon as not deſerving a ſerious examination; but, my intention here, is to propoſe a new idea, which a new principle of mine has ſuggeſted to me. From the very beginning I was convinced, that medical effluvia could not paſs through the ſubſtance of glaſs; from the beginning I experienced within myſelf, that purgative drugs held faſt within my hand, produced no effect; and now, I moreover find, that being incloſed on all ſides by my hand, it was impoſſible that they could be electrified. Nor did I, at any time, entertain a ſuſpicion, that ſparks ſent through medical drugs, could carry along with them the effluvia of

of theſe drugs through the metallic conductors. Dr. Franklin p. 83.) made his rubbing cuſhion draw its electricity through a Chain, which was dipped in oil of turpentine, and received it from another Chain, alſo dipped in the ſame oil, through a ſtratum of that oil, about an inch thick, but he could not perceive, that the leaſt effluvium was carried off by the fire.

661. The only point with regard to which, I entertain any ſuſpicion on that ſubject, is whether ſparks excited immediately from medical ſubſtances, can introduce effluvia from them into the ſkin of thoſe who excite them, either with their finger, or any other part of their body. In fact, it is a conſtant obſervation, that ſparks drive deferent vapours from bodies, and imprint them on the ſurface of thoſe bodies to which they ruſh. I ſee that Dr. Franklin has, without obtaining any effect, tranſmitted through his body ſeveral diſcharges of a bottle, within which, inſtead of water, he had put a ſtrong purgative liquid. I do not know whether he excited the diſcharges by touching the hook as uſual, in which caſe the experiment would prove nothing againſt my ſuſpicions, or whether he excited the ſpark immediately from the ſurface of the purgative liquid; nor would even this latter experiment, ſuppoſing it failed, afford an univerſal concluſion againſt my idea, ſince a man's ſkin may refuſe admittance to the purging particles, in the ſame manner as it refuſes admittance to the ſalt of ſea-water. I do not, however, pretend to affirm any thing with regard to any particular ſubſtance; I only inquire whether the tendency of electric ſparks to drive deferent vapours into their way, being once laid down as a fact, there would be no ſubſtance from which they could, with ſome kind of advantage, drive vapours into the ſkin of man. From the following experiment ſome light on this ſubject may probably be derived. I place a large drop of mercury in the bottom of a porcellain cup, which communicates with the Chain; I fix two pieces of gold wire equally near the drop of mercury, on each ſide of it; I conſtantly touch one of them, and thus cauſe the ſparks conſtantly to leap to it from the drop; after a number of ſparks, I find the piece to which the ſparks leaped, dyed with a ſky-blue colour; whereas, on the other wire, no ſuch mark is to be perceived.

662. III. The effects produced by the difcharge of a fmall jar on the mufcle of the cock above mentioned, clearly fhew, that copious electric difcharges fent through the body of a man, may produce very bad effects; they may diftend too much the fibres, they may *weary* them too much, &c. Such cures, befides, as may be effected by the means of fparkling electricity, do not always depend on the agitation and diffolution of ftagnant fluids, merely; they alfo often proceed from the tone which fparks can give to fibres; they may alfo be effected by a kind of ftimulus, which fparks may introduce into glands, &c. Formerly I ufed to dry the fiffures opened in my fingers by the action of cold, by means of electric fparks; but the experiment related above of the fmoke that rofe from a mufcle through which a fpark had been fent, and the aridity which took place on the membrane around it, evince ftill more clearly the virtue of electric fparks in diffolving and diffipating liquids. The replacing of a mufcle into its natural fituation, and the reftoring it to its former tenfion, manifeft the power of fparks in giving tone to a flaccid fibre. The increafed fecretion of wax in the ears, caufed by electricity, and obferved by Linnæus (*Carmichaël* Tentamen, pag. 33.) demonftrates alfo, it feems, than fparks can operate, barely by ftimulating thofe parts of the human body, which lie near thofe from which they are immediately drawn.

663. IV. Whatever may be the manner of the operation, would it not be better to direct, and confine the paffage of fparks, to the afflicted part alone, as I did on the mufcle of the cock, without affecting other parts, which have no need of fuch affiftance? Dr. Watfon ufed this method in his celebrated cure of a female child, who was afflicted with that dreadful diforder, an univerfal *tetanos*; he began with fending fparks through the mufcles of her lips, then through thofe of her neck, &c. But is it not moreover neceffary, to fend fparks through different portions of the afflicted part, when its extent is fomewhat confiderable? The fpark which, when it went only through the venter of the mufcle of the cock, could not replace it into its former natural fituation, feems to prove this precaution to be neceffary.

664. Having

664. Having received a ſmall accidental wound in one of my fingers, I tried (*Artif. Elect.* p. 128.) to receive through it a few ſparks from the Chain, and I felt at every one of them a kind of chillneſs, ſimilar to the diſagreeable impreſſion we feel in the nerves of the face, or of the breaſt, from the hiſſing of a file which is driven againſt the edge of a plate of iron. Now, is not ſuch chillneſs a warning of Nature, not to expoſe a naked or lacerated nerve, to the action of ſparks?

665. With regard to the medical uſes of the not ſparkling electricity, ſince it is generally agreed, that it does not produce any ill effects, and promotes perſpiration, nay, ſince in many caſes it has proved beneficial, it muſt be the care of the profeſſors of the art, to inquire in what diſorders chiefly the uſe of it may be preſcribed, and to examine whether it would really be worth the while to eſtabliſh public places, where electrization might be more eaſily performed, and with leſs expence, after the manner Dr. Prieſtley has propoſed it: the pavement of the room, he ſays, ought to be inſulated; and the machine ought to move by the impulſion of water, or of wind. To this I add, that under the room, ſtoves ought to be placed that might, when neceſſary, preſerve the inſulation of the room, and of the air in it; that in order to procure a ſufficient electricity to the pavement, and to the bodies of the people in the room, a number of glaſſes ought to be rubbed at once; the rubbing inſtruments ought to be improved, and ſince the amalgam of mercury and tin, by a rapid continued friction, conſumes the glaſs, and renders it rough, and conſequently incapable of performing any longer its functions, it would be better to keep the rubbing cuſhions conſtantly wet, than cover them with amalgam.

666. But would it not alſo be a very good precaution, if ſickly perſons had around them, within their clothes, the neceſſary electric apparatus; and the moving principle of the electricity, in their own uſual free motions? Very fine wool, when ſoftly rubbed with the hand, becomes ſtrongly electrified; why then would it not become equally electrified by the friction it would receive from the whole ſurface of the body, if it were uſed in lieu of linen?

 667. Even

667. Even the troublefome fenfation of the friction of wool againft the fkin, might be avoided: in winter time I wear a waiftcoat of beaver over my linen fhirt, and when I undrefs, I very frequently perceive thofe electrical figns which I formerly defcribed to Signor Vaudania, in *Art.* and *Nat. Elect.* p. 197. I think that by tying both the linen and the waiftcoat to different articulations of the body, the continual motion of it would create a conftant electricity, which to the leaft poffible trouble would join the greateft advantage; the body would, in this cafe, *fupply* the fire which the linen would *give* to the beaver.

SECT. IV. *Of the ufe which Nature can make of electricity with regard to living bodies.*

668. When I fhall treat of natural Electricity, an occafion will not perhaps offer, to treat of the ufe which Nature may make of electricity with regard to animals, I fhall, therefore, treat here of the ufe which fhe makes of it with regard to living bodies, univerfally; and furely, it feems, that Nature, who leaves nothing unemployed, and from every element and every elementary force, draws the greateft poffible effect for the completion of her ends, muft needs greatly avail herfelf of that moft active element, the electric fire, in her extenfive operations concerning vegetation, animal functions, &c.

669. In fact, it feems altogether improbable, that the perpetual inteftine motions with which all the operations of Nature are performed, fhould be executed, and that, in the meanwhile, the electric fire, which penetrates through all bodies and is of itfelf in fo high a degree inclined to motion, fhould remain in a quiet ftate; common fire, when compared to electric fire, feems a kind of a tardy, fluggifh fubftance, and of all the elements known to us, light alone equals it. I might add, that as Nature has allotted to all kinds of bodies a certain portion of electric fire, it feems that neither the dimenfions, nor the qualities of bodies, can be altered, without altering alfo the fhares of electric fire that belong to them, and without caufing the fame fire

to move and operate. I forbear here to dwell on all thefe confiderations, as well as omit feveral others, becaufe, however well-grounded they may appear, yet to our humiliation let it be faid, we know of no precife facts by which we can confirm thefe conjectures.

670. We have not yet been able to perceive the action of the electric fire, in any effervefcence, or in any coagulation, or in any evaporation. In the gem *Tourmalin* alone (whatever philofophers may have faid with regard to electrifying fulphurs and refins, by heating them) it has been found poffible to actuate the electric fire by the help of the common fire. In all other alterations, whether natural or artificial, of the ftate of the electric fire (the cafe of the electricity in the atmofphere excepted) it feems, that it always finds a place where to equilibrate itfelf, in the very fame inftant when it lofes its equilibrium in another; it feems that the one parts of fuch bodies as have their electric fire unbalanced, fupply this fame fire with a proportional capacity to lodge itfelf in, as foon as the capacity of the other parts becomes proportionally leffened.

671. I have fometimes had a fufpicion, with regard to the attempts that have been made to excite electricity by the means of evaporation, that the obfervations had not been made at diftances fufficiently great from the efficient principle of the evaporation. Signor D. *Ceca*, a moft worthy prieft in the *Superga**, who, encouraged alfo by the Signor Abate *Malingri* of Bagnolo, has for thefe two years paft communicated to me feveral obfervations he has made with regard to the atmofpheric electricity, has, at my requeft, made alfo the following trial; he raifed a moft copious fmoke under the long and lofty iron wire, which, from the cupola of the temple, communicates with the parifh church; but no

* The *Superga*, a magnificent convent and church, fituated on the top of a mountain, about four or five miles diftant from Turin, from whence it is eafily difcovered in clear weather. It is now the ufual place of the fepulture of the kings of Sardinia; it was built on the fame fpot from whence Victor Ameda, faw the battle gained by prince Eugene under the walls of Turin, in the year 1706, in confequence of a vow he made, in cafe the event of the battle proved favourable to his caufe.

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ſenſible alteration took place in the ſmall electricity by which, as it was ſerene weather, the ſaid wire was of itſelf animated.

672. With regard to atmoſpheric electricity it appears manifeſt, that Nature makes an extenſive uſe of it for promoting vegetation. I. In the ſpring, when plants begin to grow, *temporary* and electric clouds begin to appear, and pour frequent electric rains; the electricity of clouds, and of rain, increaſes afterwards in ſummer, and continues to do ſo, till that part of autumn in which the laſt fruits are gathered; ſo that it appears, that the electricity which obtains in clouds and rain, when carried to a certain degree, ſerves to promote, with regard to vegetation, the effects of common heat.

673. II. It even ſeems that electricity ſucceſſively ſupplies common heat itſelf, with that moiſture, by the help of which it actuates and animates vegetation; which, if heat acted alone, would inevitably be ſtopped. In fact, it is the electric fire that gathers the vapours together, forms clouds with them, and afterwards diſſolves them into rain; it is the ſame fire, therefore, that ſupplies the earth with the nutritive moiſture which is neceſſary to plants; and this moiſture, by melting the terreſtrial ſaline particles it meets with, by diffuſing them along with itſelf into the inmoſt pores of plants, cauſes them to grow and vegetate with ſuch admirable incomprehenſible regularity.

674. III. The common ſaying of countrymen, *that no kind of watering gives the country ſo ſmiling a look as rain*, may be explained on the ſame principle. The rainy clouds, by extending their own electric atmoſpheres to plants, diſpoſe the pores of the latter to receive with greater facility, the liquid which is ſoon to follow; and the ſucceeding drops penetrate into them the better, as every one carries along with it a portion of the penetrating dilating element.

675. I know that the regular diſtribution of water which is made by rain, alſo contributes to render it particularly uſeful; it even ſeems to me, that to each ſeaſon belong kinds of rain more or leſs laſting, more or leſs ſudden, and falling in larger or leſſer drops, according to the different kind of vegetation which, in every ſeaſon, are to be promoted; now, do not all theſe differences

chiefly proceed from the different degrees of the electricity which ſuch rains diſtribute, or rather accompany. I have the knowledge of ſeveral facts, with which I propoſe, in time, to increaſe the probability of theſe my former conjectures.

676. Beſides, the mild electricity by exceſs, which, as I have obſerved for theſe many years paſt, conſtantly prevails when the weather is ſerene, certainly contributes to promote vegetation, in the ſame manner as experiments have ſhewn us, that this is likewiſe the effect of the artificial electricity *without ſparks*. And is it not likely, that the former kind of electricity promotes vegetation ſtill better then the latter can do, ſince Nature increaſes it and leſſens it, in ſuch circumſtances, and at ſuch times, as particularly require it.

677. The influence of electricity on living animals is not ſo manifeſt, nor can it be ſo immediate. We cannot, indeed, doubt but animals, birds for inſtance, indicate the changes of weather. When I have leiſure enough to viſit the electric obſervatory which I have eſtabliſhed in the gardens of the *Valentino*, I am conſtantly informed of future changes in the ſtate of the weather, by herns which I obſerve flying from north to ſouth *(Atmoſ. Terr. Elect.* p. 268.) and thus I ſee the prognoſtic of Virgil verified. *Notaſque paludes deſerit, atque altam ſuprà volat Ardea nubem*; Nature continues to be the ſame, and conſequently obſervations are alſo the ſame in different ages. However, it is a difficult matter really to diſtinguiſh whether ſuch ſigns from animals, proceed from any particular ſtimulation exerciſed on their fibres by the atmoſpheric electricity, at the time when an alteration of its ſtate takes place, or from the joint alterations of the degree of the dampneſs in the air, of the winds, and of the electric ſtate of the clouds; which are cauſes, no doubt, ſufficient to determine in different ways the feelings and motions of animals.

678. A greater and more regular connexion ſeems to take place between certain peculiar ſenſations which ſome perſons experience at certain changes of weather, and the ſtate of the atmoſpheric electricity which cauſes ſuch changes. Such is the caſe of which Signor Mazeas ſent an account to Dr. Hales (ſee Dr. Prieſtley's Work, pag. 414.) of a perſon who was particularly ſubject to epileptic fits when claps of thunder took place; but

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though this perfon affured him, that the fear of thunder was by no means the caufe of his diforder, and that he felt a real connection between the above phenomena and his epileptical fits; yet, I muft ftill confefs, that I do not underftand how an alteration in the atmofpheric electricity an produce fuch effects.

679. A fingle confideration more with refpect to living bodies in general, I fhall add to thofe I have offered the reader, which is, that both in animals and men, that kind of motion is found to take place in a very eminent degree, which is apt to raife an electricity. From the friction that takes place between two bodies, of which the one is infulating and the other deferent, or alfo between two infulating bodies that are of a different, or even of the fame nature, but of which the one fuffers a greater friction than the other, from fuch frictions, I fay, a greater or lefs electricity is excited; now, why fhould not the ftrong perpetual friction exercifed by the blood againft the venal, or arterial veffels, alfo excite electricity? Why fhould not a fmall globule of blood, while it rubs againft a given part of an artery, diffufe into it fome of its own electric fire, or receive fome new portion from the fame? I admit, that for all this no vifible electricity will take place in the man, nor will any electric atmofphere be formed around him, fince through the deferent moifture in his body, the electric fire will continually circulate towards the place of the friction, there to re-eftablifh the equilibrium; but then it muft be obferved, that I do not mean to fpeak of an electricity that is accumulated, I only fpeak of an electricity that is fimply excited; certainly, when I rub a cylinder either internally coated, or wet on the outfide, a certain quantity of fire paffes from my hand to the cylinder, though, either the internal armature, or the external moifture, are perpetually reftoring the equilibrium which the friction continually tends to alter; and I really think, that fuch a perpetual excitation, as mentioned above, both takes place, and produces very beneficial effects, in the animal œconomy.

680. I am much inclined to conclude this chapter with laying before the reader the extenfive hypothefis, or rather conjecture of Sir Ifaac Newton, which I already mentioned in page 116 of *Art. Elect.*

Elect.) when I applied to the electric fluid what this great philosopher faid of an æthereal medium (*Newton, Opt. quæft.* xx.) May not the fenfe of fight confift in the vibrations of this medium, which are excited in the bottom of the eye by rays of light, and propagated to the place of *feeling*, through the folid pellucid, and uniform capillaments of the optic nerve? May not the motions of animals, continues he, be likewife produced by vibrations of the fame medium, which are excited by our *willing* faculty, and thence through folid, pellucid, and uniform fibres of nerves, are propagated to our mufcles, which they either dilate or fhorten?

681. The duration of *vifion*, otherwife that lafting impreffion made on our optic nerves by external objects, which continues to be felt though the objects themfelves are removed, may alfo, I think, be explained by the *phofphoreity*, which is found to be proper to electric light, as well as to common light; fo that the electric fluid harboured in the fubftance of the organs of fight, and vibrated by any light whatever, produces that durable fenfation we mention, in the fame manner as if it were produced by the action of common light.

682. The velocity with which electric fire moves, and the facility with which it penetrates into bodies, may account for the velocity and eafe with which the impreffions which it produces on our organs, are tranfmitted to the *fenforium*. Our feeling no irritation when our natural fenfations are affected, may partly be owing to ufe, and partly to the extremely little denfity required for producing thofe fenfations.

683. But, however laudable conjectures like thefe may be in themfelves, however ufeful to excite our curiofity, and fupport our patience in the inveftigation of new facts, as well as increafe our induftry in trying new experiments, yet, I think, they would become blameable, if we fuffered ourfelves to be led by them into any fettled and general conclufions; and the more fo, fince our natural lazinefs and vain-glory inclines us to rely on our fpeculations, rather than make them depend on fuch confequences as refult from facts, which are always difficultly afcertained, never

are inveſtigated but with great labour, and can only afford limited information.

C H A P. IV.

On ſparks relatively to foſſils, eſpecially.

684. AS ſoon as I had read and repeated the noble experiment made by Dr. Franklin, of ſending an electric ſtroke through a narrow ſtripe of metallic leaf, incloſed between two plates of glaſs (Franklin, p. 67.) I became ſenſible of the law which determined the place where the fuſion was to take place. In the firſt place, I obſerved the different denſity and ampleneſs of the ſtripe in its various parts, before the ſtroke was ſent; and I found the ſtripe melted in that part where I had obſerved the greateſt reſult, both of rarity in the metal, and narrowneſs in the ſtripe (*Artif. Elect.* p. 134.) II. I cut the ſtripes into different irregular ſhapes, and the fuſion always took place where they were narroweſt. III. I cut two ſtripes, the one into a rectangular figure, the other into an acute triangle, and placed the point of the latter in contact with the ſide of the former, and I always found that the point of the triangle had been melted in preference to any other part (ibid.) I concluded the experiment with reducing the above law to the univerſal principle, that the activity of the electric fire is proportioned to its denſity; in fact, it muſt needs be brought to a greater denſity in thoſe places, where it is compelled to paſs through a leſs number of deferent particles.

685. Dr. Franklin in his anſwer to Mr. Kinnerſley, of the 20th of February, 1762 (Franklin, p. 413.) has expreſſed more minutely the truth of the above law, relatively to his own experiment; which experiment alone would ſuffice to render his name immortal, ſince he has eſſentially ſhewn by it the connection of the effects produced by Nature, and of lightning, with thoſe

thofe produced by Art. A piece of tinfoil, fays he, three inches long, a quarter of an inch wide at one end, tapering to a fharp point, and fixed between two pieces of glafs, having had the electricity of a large glafs jar fent through it, received no injury in its broadeft part; towards its middle it appeared melted in fpots; where it was narrower, it had been quite melted, and about half an inch of it, near the point, had been reduced to fmoke. I ufually repeat the fame experiment, by placing the ftripe of tinfoil *f e*, which is narroweft in the middle, between the two pieces of glafs *h a*, *d e* (Pl. III. fig. 15.)

686. By the above law we are alfo undoubtedly to explain the fine experiments made by Dr. Prieftley with his brafs chain. The doctor has filled a whole fection with analyfis of moft ingenious experiments on the prefent fubject; and yet, he has concluded it with faying, that, with regard to the caufes of the phenomena, he has been able to make no conjecture worthy being communicated to the public; and, indeed, this is too much the cafe with us experiment-makers, who are apt to go through intricated mazes in fearch of what ftands juft before our eyes. As for me, as I was fo fortunate as to difcover in the beginning the above law, it has faved me much trouble: I ftitched a very thin fmall chain of iron, eight inches long, on a fheet of paper, and fent through it the difcharge of my plate. I then faw, I. That fmutty fpots had been left, not in thefe parts of the paper which the chain touched, according to Dr. Prieftley's account of the fact, but in thofe places where the rings of the chain touched each other. II. I examined all the rings in the points of their reciprocal contacts, and found them all more or lefs melted in thofe places: without the affiftance of a lens, I could perceive bright points on them; and with a lens, I perceived manifeft figns of a fudden fufion. III. With a foft file I made filings of metal, and placed a few of them, both on the naked margin of the plate G H (Pl. IV. fig. 9.) and on a piece of paper annexed to this margin; and fending the difcharge of the plate through thefe rare particles, I obtained marks on the plate and the paper, exactly like thofe I had before feen, after fending the difcharge through the fmall iron chain; therefore, the caufe of the fpots obferved by Dr. Prieftley, lies in

the fufion that takes place on the rings of the chain, where the fire is compelled to condenfate itfelf; that is, in thofe parts where the contact between the rings takes place.

687. All the other effects obferved by Dr. Prieftley, exactly correfpond with the caufes affigned above. I. Even the larger chains ufed by Dr. Prieftley, muft needs have given fome fmoke, and produced fpots like thofe above mentioned, fince the contact between the rings of thofe chains, though they were larger, was neverthelefs effected by only very few points: to this it is important to add, that Dr. Prieftley employed difcharges from very confiderable batteries. II. A difcharge through a continued equable brafs-wire, ought to leave no fpots, fince there was through its whole length a fufficient, uniform, capacity. III. It was very natural, that the chain ufed by the doctor, fhould lofe fo much as half a grain of its weight, fince part of its fubftance had been melted and driven into fmoke. IV. On the other hand, the diminution of the weight of the chain, ought not to have been greater, though the chain was longer, becaufe the refiftance the difcharge met with, and confequently its divifion, increafed proportionably to the number of the rings through which it had to pafs. V. When the chain was left at liberty, and fimply placed on the paper, its rings ought, as they did, to move forwards and get into one another, becaufe the vapour from the melted metal muft needs have parted and driven the rings from each other. VI. The paffing fpark ought to lighten moftly between the rings; that is, in thofe places where it was compelled, by the narrownefs of the fpace, to condenfate itfelf. VII. When the chain hung vertically, the light muft needs, from the uppermoft rings, have grown gradually lefs, the weight of the lower rings rendering their mutual contact more complete and *ample*. VIII. The paper, even where it was fomewhat diftant from the chain, which was horizontally ftretched over it, may very well have been ftained with fmutty fpots; becaufe, as we fhall fee hereafter, melted metallic particles, and the fmoke that rifes from them, may be driven to pretty great diftances.

688. One of the experiments of Dr. Prieftley has, I guefs, particularly contributed to conceal the truth from him. Having placed

placed a part of his ſmall braſs chain on three half-crowns, he found that the latter had been melted in the points of contact; but it was very natural that the diſcharge ſhould prefer to paſs through the three capacious pieces of ſilver, and leave, along their whole extent, the ſmall capacity of the chain; and in thoſe places where, from the chain, the diſcharge got into the ſilver pieces, as well as in thoſe where, from the ſilver pieces, it got back into the chain, it muſt needs have melted them, in the ſame manner as, in the former experiments, it melted the rings.

689. When Dr. Prieſtley held a part of the chain on his hand, the latter was ſtained in the ſame manner as the ſheet of paper had been, and he only felt a great heat from the chain, which laſted but a ſhort time. This was becauſe the diſcharge did not leave the chain to get into his hand, which was of a much leſs deferent nature, as it had done with reſpect to the ſilver pieces, which were equally deferent with the chain, and more capacious; if the diſcharge had really got into his hand, it would undoubtedly have burnt it, and not barely ſtained it with the vapour of the melted metal.

690. I ſhall conclude with an experiment of Dr. Prieſtley, which, in my opinion, completes the demonſtration of the explanations I give here; it conſiſts in placing the chain through which the diſcharge is ſent, within a tube of glaſs. Dr. Prieſtley has obſerved, that four rows of ſpots took place within the tube, in the ſame manner as if they had been effected by four chains. I have repeated this experiment: with ſhort ſticks I faſtened a ſmall chain within the orifices of the tube of glaſs I uſed; I then ſent through it the diſcharge of two large jars of cryſtal, and I obtained the above four rows of ſpots: the bare ſight of which pointed out to me the cauſe from which they proceeded; every ſtain directly correſponded to the middle point of the angles which the rings formed between themſelves; and the vapour which aroſe from the metal that had been melted in the common *apex* of the four angles formed by every two rings, that is in the common place of contact between them, had been thrown in four oppoſite directions.

691. This fuſion of metals in their common points of contact, is no more than a particular inſtance of the univerſal law formerly ex-

preſſed,

preſſed, which is, that a ſpark always melts metals in thoſe places where it enters into them, and gets out of them; or again, it is only a conſequence of the ſtill more univerſal law, that a ſpark operates on each ſubſtance proportionally to its own denſity, and that it grows condenſated proportionally to the narrowneſs of the paſſage through which it is compelled to paſs. When a very copious diſcharge is excited, both the coating of the plate, and the conducting bow, are melted in ſeveral ſmall points of their ſurface; if the diſcharge be ſent through a quire of paper within which a leaf of tinfoil has been placed, the latter is melted the more completely, as the diſcharge, in this caſe, ruſhes more united from the leaf to the conducting bow; but if two or three holes are bored in the quire, then the fuſion becomes leſs, becauſe the diſcharge paſſes more divided. If you ſend a ſpark through the point of a large needle, perpendicularly fixed on a metallic ſurface, a longer part of it will be melted, in proportion as the ſpark will be more united and condenſated; and when I obſerve the effect of lightning, I can always find marks of its paſſage through bodies, both in the place of its entrance, and of its getting out, and in general in thoſe places where it has flowed through very narrow parts of the ſaid bodies.

692. But in order more completely to underſtand this law of the places of *fuſion*, two others muſt be added to that of the *condenſation* being in an inverſe ratio to the ſections of the bodies, through which the fire is to paſs. The one of theſe laws is, that the reſiſtance increaſes proportionally to the length of the medium through which the paſſage is effected; this law I have mentioned in a former place. The other law, which I do not know that any body elſe has mentioned, is, that the reſiſtance oppoſed to a ſpark by a body through which it is ſent, is greater in proportion as the place of the paſſage of the ſpark, lies deeper in the ſubſtance of the body, under its ſurface.

693. To aſcertain the truth of the firſt law, I placed a ſtripe of gold or ſilver leaf, one line broad, between two pieces of glaſs; the diſcharge of ſuch a coated plate as that which I commonly uſe, melted it only in a few places, and a great remnant of charge was left in the glaſs; when the ſtripe was ſhortened, and its extremities,

at

at the ſame time, were made wider, ſo that its former width of one line only obtained in a ſpace of a few lines, the diſcharge was then much louder and more perfect, and the fuſion more complete; which proves the truth of the above aſſertion, that the reſiſtance which ſparks meet, is greater in proportion as the length of the narrow paſſage left to them, is greater. Dr. Prieſtley had before made ſimilar obſervations; with his large batteries he could melt an iron wire, three, ſix, or nine inches long, according to the ſtrength of the diſcharge; but could only make red-hot a wire of a greater length.

694. The ſecond law, that the reſiſtance is leſs, in, or near, the ſurface of deferent bodies, is entirely conſiſtent with reaſon. In fig. 7. and 8. of Pl. V. two ſections of a cylindric body are repreſented; the one perpendicular to the axis, the other parallel to it; the direction of the ſmall white lines expreſs the direction of the pores through which the electric fire is to inſinuate itſelf; now, when the ſtream of it will be ſo copious as to make it neceſſary for it to diſtend thoſe pores, it is evident, that it will eaſily effect it near the ſurface, becauſe the ſolid parts of a body are connected with a ſucceſſively leſs number of other particles, in proportion as they are placed at a leſs depth within the ſubſtance of the ſame, &c.

695. This explanation is the ſame with that given above, why ſtrong copious ſparks almoſt entirely flow along the ſurface of bodies which are imperfectly deferent. Metals are the moſt deferent bodies we know, yet, if we leſſen their dimenſions, and increaſe the ſparks ſent through them, they become in ſome degree *reſiſtent*, and a greater portion of the fire is then obliged to flow along their ſurface, and effects a greater alteration in their ſuperficial parts.

696. In fig. 6. Pl. IX. I repreſent the extremities A *a*, B *b*, of two braſs rods, rounded in *a* and *b*, and bored along their axis, though ſomewhat obliquely, from *a* to E, and from *b* to F; I inſert into theſe two holes an iron wire, the eighteenth part of a line thick, and faſten it by twiſting it round the ſaid rods in E and F, ſo that the portion *a b* be three, ſix, eight, or more lines long; I ſurround both the wires, and the extremities of the rods, with

melted

melted ſealing-wax, which I pour around them; I ſend ſeveral ſucceſſive ſparks from my coated plate through the wire, when I ſee flaſhes of light take place within the ſealing-wax, and this light increaſes according as I ſend a greater number of ſparks. After eight, or more diſcharges, I break the cylinder, and find the ſurface of the wire black and rough; I ſcrape it with my nail, and a duſt falls on a leaf of paper, which has all the characteriſtics of ſcoriæ of iron, and are, I find, intirely ſimilar to thoſe which may be taken from the remains of the iron wire, which was ſtruck by lightning at the *Superga*. Even when I ſend through a ſimilar wire, though not incloſed, a diſcharge leſs than what is requiſite to melt it, I find that it is coloured blue, and that it is blackened, when the diſcharge is ſomewhat intenſe; now, the blue colour indicates only a ſmall degree of alteration of the ſuperficial parts of the metal. When the wires have been blackened, and the duſt around them ſcraped off, they appear bright underneath, like the iron wire at the *Superga*; it ſeems as if a part of the phlogiſtic driven from the metal, had been fixed on its under ſurface.

697. The fuſion of ſtripes of metallic leaves between pieces of glaſs, proves the ſame things: in proportion as the glaſſes are leſs preſſed againſt each other, ſtains from gold or ſilver leaves, conſiſt of longer tracts, or ſtripes, perpendicular to the direction of the paſſage of the ſpark.

698. But let us now treat more accurately of the effects of this fuſion, or alteration whatever, in metallic ſtripes. Firſt, a ſpark when it melts ſuch ſtripes, from *deferent*, renders them *inſulating*. Send, as in numb. 693, a ſtrong diſcharge through a long and uniformly narrow ſtripe of gold or ſilver leaf, the crack, as was ſaid above, will be weak, there will be a great remnant of charge, and though you preſent afreſh the conducting bow to the plate, this remnant will almoſt intirely refuſe to paſs. If you charge again the plate, you ſhall find that the ſame ſtripe can only tranſmit a ſtill ſmaller portion of the charge, ſo that the remnant of it might now very eaſily melt another ſtripe; the meaning of all this is, that the particles of the ſtripe which have been turned into ſcoriæ, are no longer deferent, and that the whole ſtripe is become in great meaſure inſulating.

699. From this principle muſt be explained the impoſſibility of ſending a diſcharge through the ſtains from metallic particles, that take place on a plate of glaſs; a fact, which has perplexed Dr. Prieſtley. Theſe ſtains are become inſulating, exactly in the ſame manner as the gold or ſilver ſtripes above mentioned.

700. Another effect of ſparks with regard to metallic ſtripes, is to drive their particles into the very ſubſtance of the glaſs, which latter it really melts. I have a viſible demonſtration of this fact in a cylinder of ſolid glaſs, an inch thick, nine inches long: its uſe was to fix one of the extremities of the long iron wire with which they uſed, at the Superga, to obſerve the ſtate of the atmoſpheric electricity, and it was covered with a cap of tin ſharpened at its top. Two ſparks of the intenſe lightning, which in laſt September ſtruck this apparatus, flung from the cap two pieces of the metal, and drove them into their way along the cylinder of glaſs, where they imprinted them on the ſurface of it, at the diſtance of about two inches and an half from the center of the cap; thoſe ſtains from the metal are very conſpicuous and bright, and are imprinted on all thoſe places of the cylinder on which the ſpark as it were reſted itſelf: thoſe places are indeed very remarkable; the glaſs is melted to a ſenſible depth, and the waves that manifeſt ſuch fuſion, proceed, gradually diminiſhing, to the ſpot, where the melted metal is fixed into the glaſs. Each of theſe ſpots reſulting from the metal and glaſs melted together, is diſpoſed in a longitudinal direction, on the ſurface of the cylinder, and they are about five lines long, and three or four wide.

701. Every-body may, by uſing ſtrong diſcharges, aſcertain the truth of theſe facts. I have already obſerved, how, on the ſurface of a piece of glaſs, I fix filings of metal well preſerved and whole, which would not be poſſible, unleſs the glaſs were ſuperficially melted; beſides, if you examine with your nails or fingers, the place where the narrower part of a metallic ſtripe has been melted by a ſpark, and driven into the glaſs, you ſhall find the latter ſomewhat hollow, and ſenſibly rough.

702. It is, therefore, by no means ſurpriſing, that the metallic particles thus driven into the glaſs, reſiſt the action of aqua fortis, or aqua regia: they are driven to a ſenſible

 depth

depth into the glafs, and are coated and protected by a kind of vitreous fur or veil. It feems that the above particles, when entering into the glafs, draw around them the fluid particles of the latter's furface, and are coated by them in confequence of the remarkable chymic affinity between glafs and metallic fubftances.

703. But fince this heating virtue or force of fparks is fufficient to melt parts of plates of glafs, why could it not fhatter to pieces the plates themfelves. Dr. Franklin, the inventor of this experiment, firft faw plates of glafs reduced by the effect of a ftroke, to a kind of duft like coarfe falt, when thofe plates had been firft ftrongly preffed againft each other in a prefs. I obferve, that plates very feldom remain entire, when I have faftened them to each other with ftrings, and compreffed them with weights. As Dr. Franklin ufed metallic ftripes uniformly narrow in their whole length, the glafs plates were uniformly broken throughout the length of the ftripes; as to me, as I commonly ufe ftripes made narrow only in fome fmall part of them, I obtain more expreffive fractures; that is, the plates are fhattered into four, five, or fix fharp pieces, which exactly converge towards the point where the ftripe is narroweft. I have, among others, a plate of glafs on which are four fiffures, which run from the above point to the very perimeter of the plate, the whole plate having remained united; it affords a demonftration, that pieces of glafs are broken in that place where the fire is moft condenfated.

704. In order to fhew that moifture is not neceffary to enable a fpark to break a fheet, or piece of glafs, as it was for breaking the fmall tube of glafs mentioned in a former chapter, I very carefully dried and polifhed the pieces of glafs I ufed; I warmed them, and placed brafs leaves between them frefh taken from the book; and always, when I had previoufly taken care to tie and prefs them, I found them broken.

705. To avoid the danger of having any unctuous or moift particles adhere to the brafs leaf, I put inftead of it, a few very fine filings between the pieces of glafs, and in this cafe alfo they were broken.

706. Not-

706. Notwithſtanding the precautions juſt now mentioned, I was ſtill afraid leſt ſome unperceived moiſture had been the real cauſe of the above phenomena, but the following accident has removed my ſuſpicion; and, at the ſame time, has explained to me the reaſon of all the above facts. Having placed a ſtripe of gold between two pieces of glaſs in the manner mentioned above, and ſent a diſcharge through it, I ſaw one of the two pieces marked with fiſſures; having looſened it, I found that the fiſſures only obtained on the outer ſurface of it, and did not reach the inner ſurface; whence I concluded, that the heat communicated by the ſpark to this inner ſurface, had, by dilating it, prevented its being broke; and as the heat had not yet reached the outer ſurface (it muſt be remembered that glaſs which is not pervious to electric fire, is little more ſo to common fire) it remained in its uſual ſtate of ſtiffneſs and contraction, while the other ſurface of the piece of glaſs was dilated; and thus it was broken. Fig. 14 of Pl. IX. repreſents the plate with the fiſſures in its outer ſurface, the points indicate the place occupied by the gold leaf, cloſe to the inner ſurface.

707. The third viſible effect produced by ſparks, is to alter the colour of bodies, I. In holding the piece of glaſs againſt the light, the ſtain left by gold appears of a purpureous colour; in examining the ſame directly oppoſed to light, it appears of a golden purpureous colour; the golden colour is much dilated, and ſomewhat bright, yet it prevails on the purpureous dye; but if the other ſurface of the piece of glaſs be inſpected, the purpureous dye prevails on the golden colour; on whatever ſurface of the glaſs the ſtain be examined, a grey bottom is always to be perceived through the purpureous or golden dyes.

708. By holding the piece of glaſs againſt the light, in order to obſerve the ſtains made by melted ſilver, they appear of a colour which is between yellow, green, and coffee; in looking directly upon them, they appear of a grey cinereous colour, but with bright white points; in looking on the other ſurface of the glaſs, they appear of the ſame colour as mother-of-pearl.

709. In holding the piece of glafs againft the light, the ftains made by gilt copper, appear compofed of red points, like minium, fpread on a grey bottom; in looking directly upon them, you perceive a mixture of a grey colour, with red points, fomewhat bright, but rarer than in the former cafes; when the ftains are obferved from the other fide of the plate, the red colour predominates.

710. In examining through the glafs, ftains made by melted tin, they appear of a dirty cinereous colour; being directly looked upon, they appear of a deeper cinereous colour, fomewhat bright; being examined from behind, this brightnefs difappears.

711. I have defcribed the above appearances in the manner they were effected by the difcharge of my ordinary coated plate. Sparks more or lefs intenfe, I have obferved, produce a few differences, but they, however, do not introduce any material change into the appearances above defcribed.

712. From thefe facts we may, I think, conclude, that the metallic particles contiguous to the glafs, fuffer, in confequence of this contiguity, a more perfect fufion; this appears evident from the purpureous colour perceived when a gold ftain is examined from behind,—from the colour of mother-of-pearl, perceived when a filver ftain is obferved from behind,—from the more intenfe and copious red colour, which is to be perceived when a copper ftain is obferved from behind,—and from the vanifhing of the bright points on a ftain produced by tin, when obferved likewife from behind.

713. It refults, in the fecond place, that efpecially in ftains made by filver, copper, and tin, a colour rifes which has a mixture of a grey colour in it; I fay, *efpecially*, becaufe even in ftains made by gold, I perceive, when I look directly upon them, fomewhat of this grey colour mixed with the reft; and it is by obfervations fimilar to this, that I was led to fay (pag. 134 of *Art. Elect.*) that ftains of various metals had their bottoms of pretty much the fame colour.

714. And now thinking, that the affinity of glafs with metals, might perhaps contribute to form the colour of fuch bottoms, I thought of comparing ftains made on glafs, with thofe made

on

on paper. I adjufted, as formerly, ftripes of different metals between pieces of glafs, but, between one of the pieces of glafs and the ftripe, I inferted a fheet of paper, and thus obtained very equal ftains, both on the glafs and the paper; here follows the defcription of the ftains which conftantly took place on the paper. I. A ftain made by gold, was grey and purpureous. II. The ftain made by filver, grey and cinereous. III. That of tin, of a grey ferrugineous colour. IV. That of copper, cinereous grey, much like that produced by filver. After thefe obfervations, I think I may again propofe my former queftion (*Art. Elect.* p. 135.) Is not this grey colour arifing from the fufion of all metals, a fign of fome conftituent principle common to them all? Are not the different colours perceived in the ftains from different metals, figns, or fymptoms, of the different fubftances, or principles, of which they are formed?

715. The fourth vifible effect produced by fparks on metallic ftripes, relates to the diftribution of the latter's melted particles. Univerfally, in gold, filver, copper, tin, the melted particles are diftributed into furrows, or dafhes, which are tranfverfal to the direction of the fparks, fo that there is an alternate feries of lines, which are more deeply coloured in confequence of the particles of the metal being more copious, and more completely melted, and of lines in which fuch melted particles being more rare, the glafs has been left more pellucid. II. Thefe alternate dafhes, or lines, are thinneft in gold ftains, fo that to the naked eye they appear as if they were one continued ftain, but with a lens they may be eafily diftinguifhed, except in thofe places where the fufion has been very copious; the fame may be faid of the alternate dafhes in filver ftains: in copper ftains, the dafhes are more difcernible; in ftains from tin, they are thicker and more apparent. But, befides thofe *primary* dafhes, which we might call fimple dafhes, there are others that are thicker, and, as it were, compofed of feveral fimple ones. This obfervation ferves more amply to confirm my conjecture, expofed in num. 566, that fparks have alternate places of greater and lefs denfities, according as the refiftences which alternately, ftand firm, and yield, occafion it: univerfally, fparks melt bodies more complete-

ly,

ly, where, from more copious deferent particles, they rufh into fcantier ones; and, likewife, where they gather themfelves into a lefs fpace, in order to rufh into a wider fyftem of deferent particles; thus, a metallic ftripe is melted more completely, in that part of it which is joined to that which grows wider at once.

716. To the fubject of the *diftribution* of the melted parts of metals, belongs alfo the confideration of the lateral fmoky effluvia from them, which lie tranfverfally to the paffage of the fpark. In gold and filver, thefe tracts are feen as if covered with a kind of exhalation. Examine attentively a piece of glafs on which gold has been melted, and the lateral tracks will, in every point, retain the violaceous purpureous colour; only, this colour will ufually appear more languid, as the diftance from the center of the ftain will be greater, in confequence of the greater rarity of the particles; the fmoky tracks from a filver ftripe, will appear of a fky-blue colour, inclinable to green, which likewife gradually vanifhes.

717. Even from the beginning, I melted a ftripe of metal leaf, expofed to the open air, either placed upon a piece of glafs, or fufpended from the head of the conducting bow (*Art. Elect.* p. 134.) but the fufion was, in thefe cafes, attended with a few circumftances different from thofe above, which could not but take place. Place a ftripe of metal leaf upon a plate, with another plate cover only an half part of the ftripe, and then fend the fpark through that part of the ftripe which is preffed between the two pieces of glafs, deep and lafting ftains will arife from it;—from the other part of the ftripe, a ftain will refult, which will be much more of the nature of fmoke, and will eafily be rubbed away by the hand. If the copioufnefs of the fpark be increafed, ftains will take place, even in the open air, pretty denfe and lafting.

718. By increafing the fparks, Mr. Kinnerfley brought them, from melting metallic leaves, to render red-hot, even in the open air, an iron wire the fifteenth part of an inch thick; the fparks could even drive it into fparkling drops, and deftroy it. I attempted to repeat the experiment, but found the difcharge of two coated plates infufficient.

719. I then attempted with the difcharge of a fingle plate, to lengthen an iron wire, which was a fixteenth part of a line thick,

thick, or thereabout. In fig. 5. Pl. X. is reprefented the apparatus of which I made ufe for that purpofe. I. Upon the horizontal table LM, the brafs rule, AB, to which the dented wheel RM has been fitted, is fo placed, as to defcribe an arch of a circle from the center A; to that end the brafs rule is raifed on two wheels or pullies, QH, which move on a plate of brafs well polifhed, and very even. II. The arch RM makes the fmall wheel B move, which carries the index BI. III. I infulate the whole on a glafs plate, which I place on a coated fheet; I put a candleftick on the latter, in *e*, and then adjuft the iron wire to the brafs rule in D, and to the candleftick in Q. IV. Things being thus difpofed, by prefenting one end of the conducting bow to D, I make the fpark rufh through the wire DQ, eight inches long; and, in this very inftant, the index BI runs along the arch I*i*, which is about the twentieth part of the circle; the reafon is, becaufe when the wire DQ is lengthened by the fpark, the fpring EF becomes enabled to drive forwards the brafs rule AC. Having more accurately calculated the quantity of the motion, I found it to be a ninth part of a line.

720. From this experiment, I alfo perceived how rapidly the fmall degree of heat in the wire, acts, and caufes the brafs rule to move, and how rapidly the fame afterwards vanifhes. When you watch with attention the motion of the index, in order to fee it reach the point *i*, the fhorteft moment of abfence is fufficient to make you lofe fight of it, and you perceive it again, only when it returns to *u*: it is only in this laft part of the arch which the index defcribes, that it moves with flownefs and regularity.

721. A degree of heat more vehement, produces a lengthning of the wire, which is greater and more lafting. Mr. Kinnerfley fent the difcharge of thirty-fix bottles through an iron wire two feet long, vertically drawn by a weight; the wire became red-hot, and was lengthened a full inch more than it was before; a fecond difcharge not only lengthened the wire four inches, but broke it in the middle (Franklin, p. 392.) By means of the fame battery, Mr. Kinnerfley likewife melted an iron wire before Dr. Franklin,

Franklin, and fo inflamed it, that drops fell from it, and for a while continued burning on the floor.

722. Mr. Canton and Dr. Prieftley repeated afterwards this interefting experiment. Dr. Prieftley, with his large batteries, drove iron wires into fparkling drops to the diftance of feveral feet; he could even deftroy them, though dipped in water, and the water was plentifully fcattered away. Dr. Prieftley obferved, that the wires refifted more againft being driven into drops, in proportion as their length was greater; that iron wires threw burning drops which fparkled more vividly and *amply*, than brafs wires; that when the difcharge was not very intenfe, feveral of the drops into which the metal had been fcattered, affumed, when they cooled, the fhape of fmall balls.

723. With a difcharge of three large cryftal jars, I can melt an iron wire, the fixteenth part of a line thick; and the ftumps of it, which remain hanging from the two brafs rods, continue to lighten, and throw fparkling drops during a fenfible fpace of time afterwards: thefe drops are exactly like thofe produced by throwing iron filings through the flame of a burning candle. The fmall globules produced by an iron wire, when melted by a middling difcharge, are like the blackifh globules that are made to fall on a fheet of paper, by ftriking a piece of fteel with a flint; they confift of iron nearly reduced to the ftate of fcoriæ.

724. Nor is it neceffary, in order to afcertain all the above facts, to make ufe of a very confiderable battery; fome few iron filings placed in a regular row, on the margin of the naked fheet of glafs HQ (Pl. IV. fig. 9.) will, by means of a fingle difcharge of the coated plate AB, exhibit fparkling drops, fmall globules, &c. I ufe the word to *melt*, becaufe a more proper expreffion is wanting in order to exprefs the above effect of electric fparks on metals; for, certainly, copper, tin, lead, and iron, are rather deftroyed than melted, becaufe their phlogiftic is driven off, and they are turned into fcoriæ, into calces, or even into glafs; nor are the particles of gold, or filver wires, fimply melted, this word only expreffing a ftate of fluidity; they are fcattered into fmall disjointed particles.

725. Hence

725. Hence we may derive a solution of the queſtion propoſed by Dr. Prieſtley (p. 729.) who could not find any reaſon why ſome metals are driven into drops, or vapours, by electric ſparks, while others are only reduced to a ſtate of fluidity. He ſent diſcharges through pairs of wires of different metals, that were equally thick, equally long, and joined together; when he found, I. That the diſcharge completely diſſipated an iron wire, and left a braſs wire intire. II. That another diſcharge intirely diſſipated another iron wire, and left a copper wire intire. III. That another diſcharge diſſipated a copper wire, and left a braſs wire intire; though, thoſe wires which were preſerved intire, were, the doctor remarks, ſomewhat melted in the place where they were in contact with the other wire. IV. That a certain degree of charge ſcattered a braſs wire into vapour, and left a ſilver wire intire. V. That a certain quantity of charge deſtroyed a braſs wire, and left a gold one intire. VI. That a certain degree of charge deſtroyed a ſilver wire, and left a gold one entire. From theſe obſervations, Dr. Prieſtley concludes, that metals may, with reſpect to their power of reſiſting the effects of the electric fire, be ranked in the following order: I. Iron; II. Copper; III. Braſs; IV. Silver; V. Gold. Dr. Prieſtley adds another experiment, by which he proves, that lead is more eaſily deſtroyed than tin.

726. This power of metals of reſiſting ſparks, follows a very regular order, even with reſpect to iron, which, though it is of itſelf the moſt difficultly melted of all metals, is more eaſily deſtroyed by electricity than copper, braſs, &c. the reaſon is, becauſe the effect of ſparks is not ſo much to produce a complete fuſion of the inward ſubſtance of metals (ſo that their phlogiſtic be preſerved, and the metal only brought to a ſtate of fluidity) as to drive off this phlogiſtic from the ſuperficial parts of them; now, the facility of this diſſipation of the phlogiſtic of metals, is not proportioned to the facility of their fuſion, but to the facility with which their ſuperficial parts may be deprived of this phlogiſtic; and it happens that the leſs perfect metals, when expoſed to the action of common fire, loſe their phlogiſtic, with a facility that exactly follows the order in which they reſpectively yield to the action of electric ſparks. Lead may be turned into ſcoriæ more eaſily, and to a

greater depth, than tin ; tin more eafily, and to a greater depth than iron ; iron fooner, and to a greater depth than copper : the fame obfervation may alfo be applied to the more perfect metals ; copper, when reduced to a ftate of fufion, lofes more of its fubftance than filver ; and filver alfo fuffers a greater lofs than gold, which fuffers none, or at moft an exceedingly fmall one.

727. Hence may be explained the reafon why fparks, when not very intenfe, only deftroy the fuperficies of iron wires : the reafon is the fame as that for which common fire gives, according to its different degrees of intenfity, different colours to iron, and at laft brings it to the black dye of fcoriæ ; a ftate into which it can more completely turn the fuperficial parts of it, than the internal ones, as the phlogiftic of the latter is protected by the external parts. According to this principle, we fee brafs-founders, and efpecially iron-fmiths, endeavour to protect the fuperficial parts of their metal, by keeping it covered with bodies extremely replete with phlogiftic.

728. A more complete knowledge of the alterations produced both by common fire and electric fire in the fubftance of iron, and efpecially in its phlogiftic, with which it feems remarkably to abound, would throw a great light on the connection between electricity and magnetifm, and greatly contribute towards unveiling this important, though dark, myftery of nature.

729. It is a fact extremely well known, *that an electric fpark of a certain degree of intenfity, fent through a needle, gives it, takes from it, or even inverts in it, the magnetic direction.* This fact is more fimple, and has a more extenfive relation to our mundane fyftem, than has been at firft imagined, fince *electric fparks can, in an inftant, alter the magnetic direction of pieces of iron, in the fame manner as particular pofitions of the fame, can only effect after a long courfe of time.* So that it appears, that a ftrong fpark does nothing more than introduce in an inftant, the fame alteration in the reciprocal fituation of the particles of iron, as is flowly effected by the univerfal imperceptible current of the fame fluid, which moves within our fyftem with a given direction. (*Atmof. Terr. Elect.* pag. 260.)

730. The

730. The following is an account of the manner in which I difcovered and afcertained the law, that electrical fparks introduce the magnetic direction into needles, according to the pofition in which they were placed when they received thefe fparks. If the fpark, faid I, turns the point of the needle into which it gets, towards the north, whatever may have been the fituation of the needle, this is a particular effect produced by a particular action of the fpark, which acts according to its own direction. This important point, in which the whole theory of electricity is concerned, and which furely is not indifferent to the fcience of magnetifm, has fo much attracted my attention, that I took myfelf to examine pieces of iron, ferrugineous ftones, and bricks, which had been ftruck by lightning, and of which I had before obferved the pofition, in order to form my conjectures with regard to the direction of lightning; and I faw that, when the direction of the ftroke of lightning had been from north to fouth, or from fouth to north, all the above bodies attracted that point of the needle which verges towards the fouth, with fome of their fouthern, or alfo of their northern parts; I, therefore, began to fufpect that the magnetic direction introduced by lightning, depended not only on the direction of the latter, but alfo on the pofition of the bodies which had been ftruck *.

731. Then I paffed to experiment with artificial fparks; and, in order to avoid the tedious tafk of fetting needles afloat in a cup filled with water, as well as the danger of falling into errors, I got feveral needles to be made, as reprefented in Pl. X. fig. 6. to which a fmall *cap* made of cryftal, was adapted; then I reafoned thus, if a fpark introduces a magnetic direction into a needle, that depends on the pofition which the needle had when it was ftruck, no direction can be given to a needle placed at right angles with the magnetic meridian, that is, placed equally between a north and a fouth direction: in order to afcertain this, I ufed the difcharges of two large jars of cryftal. I. I ftruck a needle placed

* If the above paragraph, on the contents of which the author feems to lay a particular ftrefs, had been more clearly expreffed in the original, the tranflation of it would alfo have been fomewhat clearer.

as above ſaid, perpendicularly to the magnetic meridian, and ſent the ſpark through the eaſt point L; but how aſtoniſhed was I, when having placed the needle on a bodkin, and covered it with a glaſs bell, it conſtantly aſſumed the ſame direction it had when I ſtruck it! II. I placed the needle in the ſame poſition as before, and now introduced the diſcharge through the weſt point P; and having placed it again on the bodkin, it kept its former direction. III. I placed the needle on the ſame line as before, but inverted it; ſo that the point P now aimed at eaſt, and the point L at weſt; and after ſending a diſcharge, I found that the needle was now directed, the point P to eaſt, and the point L to weſt. IV. Having again placed the needle in the ſame poſition as I had juſt now done, I now ſent a diſcharge from weſt to eaſt, and found that the needle ſtill directed its point P to eaſt, and its point L to weſt. I muſt confeſs, that it took me a conſiderable time before I could unravel the myſtery; yet, the reaſon of it was the very ſame for the finding or aſcertaining of which I had been firſt induced to make the experiment; that is to ſay, the ſpark by running through the needle, when placed at right angles with the meridian, gave a direction towards the north to that part of it, and of the *cap* in the middle of it, which lay on the north ſide, and gave a direction towards the ſouth, to that part of the needle, and of the *cap*, which lay on the ſouth ſide.

732. V. I placed the needle directly upon the meridian, and having ſent the ſpark from north to ſouth, I found that point of the needle directed towards the north, which had been placed towards the north. VI. I placed it again on the meridian, and having ſent the diſcharge from ſouth to north, I found that the needle was ſtill directed in the ſame manner as before. VII. I again placed the needle on the meridian, but inverted it, and a ſingle diſcharge, ſent from ſouth to north, inverted the direction of the needle, and the point which before verged to the north, now verged to the ſouth. VIII. I again placed the needle in the inverted poſition juſt now mentioned, and another diſcharge, though now ſent from north to ſouth, confirmed the laſt direction of the needle.

733. Laftly, I placed the needle vertically; and, IX. A difcharge fent through the upper point, caufed the needle to direct its lower point to the north. X. I now introduced the difcharge through the lower point, and the fame direction was confirmed. I afterwards placed the needle in the fame vertical pofition, but inverted, and a fingle difcharge fent through the lower point, inverted alfo its direction. XII. Having again placed the needle in the latter pofition, and fent the difcharge through the other, that is, through the upper point, ftill the latter direction was confirmed.

734. Hence we may univerfally conclude, that *fparks and lightning do not introduce a magnetic direction into bodies, that be determined by their own particular direction when they enter into thofe bodies, or get out of them*; in fact, that point of the needle which aimed at north when the ftroke was fent, is always afterwards directed towards the north, whatever may have been the direction of the fpark (whether northwards or fouthwards) in rufhing through it; and (what is very fingular, and may have been the caufe of the miftakes of former obfervers) a fpark fent with any direction whatever, through a needle placed at right angles with the meridian, always caufes thofe parts of it to be directed to the north, which were placed on the north fide when the ftroke was fent. *But fparks and lightning introduce a magnetic direction into a needle, or other piece of iron, or into a ferrugineous body, according to the pofition which thofe bodies were in, when they received the ftroke*; and, indeed, it is a fact very well known, that pieces of iron placed on the magnetic meridian, gradually acquire the property of directing towards the north, that part of them which aimed at north; and that, of pieces of iron vertically fufpended, that part is afterwards directed to the north, which hung loweft when they received the ftroke.

735. We muft not, however, infer from thence, that the *polarity* of pieces of iron produced by a fpark, or by lightning, is wholly to be attributed to their pofition; fince, I. Lightning, as we fhall fee hereafter, may create a far greater magnetical power of fupporting weights, than any which may arife from the pofition alone of bodies. II. No brick, no ferrugineous ftone,

can

can acquire from its poſition alone, the power of attracting a magnetic needle, but may receive it from a ſtroke of lightning. III. The poſition alone, as I mentioned above, does not give ſo quickly a polarity to a piece of iron, as a ſpark can do, nor does it give it to ſmaller needles, in ſo high a degree.

736. Are not theſe peculiar effects of the electric fire with reſpect to magnetiſm, ſo many proofs which corroborate my former conjectures, that the peculiar magnetic force obſerved in *load-ſtone*, is to be attributed to either atmoſpherical, or ſubterraneous, ſtrokes of lightning; and that the *univerſal ſyſtematical* properties of magnetic bodies, are produced by an univerſal ſyſtematical circulation of the electric element? It is true, magnetiſm is excited by other means than electricity, ſuch as heating, hammering, rubbing, breaking, &c. but is not common fire or heat, alſo excited in a number of different ways; and yet, it is always found to be the principal agent, and, as it were, the author of its own phenomena.

727. But of theſe conjectures, I ſhall treat more at large in *Nat. Elect.* now, proceeding farther on the works of Art as to electricity and magnetiſm, I declare, that I never could by any method whatever, introduce any ſign of direction, either towards the pole, or towards ſimilar metals, in needles made of gold, of ſilver, or of copper. I do not mention this in order to confute the opinion of thoſe who, on that ſubject, quote the authority of Aldovrandi in his *Metallurgia*, which indeed in theſe days, would be needleſs; but only, in order to obſerve, that the property of *magnetiſm* is wholly peculiar to iron; this metal is remarkably replete with inflammable ſubſtance, and whenever a too intenſe ſpark drives this ſubſtance from it, it inſtantly loſes every diſpoſition to acquire magnetic properties; which circumſtance greatly increaſes my ſuſpicion, that ſome igneous principle is the cauſe of ſuch properties, and that this principle produces them in bodies, by modifying, after ſome peculiar manner, the inflammable ſubſtance which they contain.

738. But what other igneous ſubſtance is more univerſal, either with reſpect to its diffuſion, or its operations, than the electric fire? Of its diffuſion, I have treated in the firſt ſection

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of this work; I ſhall now relate the different experiments with which, in the *Atmoſ. Terr. Elect.* (p. 254.) I found means to vitrify, to calcinate, or to metallify, ſeveral bodies, conformably to their nature; and thus ſhewed, that the electric fire produces in an inſtant, the ſame alterations in bodies, which common fire only can effect gradually and ſlowly. I. I reduced the ſubſtances whether metallic, or of other kind, on which I intended to experiment, into powder, which I carefully dried. II. I put ſuch a quantity of it in a ſmall tube, half a line wide, as might form a ſmall cylinder half a line high, or more, according as the ſubſtance I uſed was more or leſs inſulating. III. I rammed this cylinder, within the tube, with two ſmall iron rods. IV. And laſtly, I wrapped the tube with ſtrong paper, that I might afterwards examine the pieces into which the tube was often ſhattered, eſpecially when I experimented on ſubſtances, from which very elaſtic vapours aroſe. V. The ſpark was uſually formed by the diſcharge of two coated plates, the ſum of the coatings of which was about ſeven ſquare feet.

739. I. Powder of Borax, thus incloſed in a ſmall glaſs tube, appeared at firſt white and opacous; after being traverſed by a ſpark, it appeared like a ſolid body, which was tranſparent, and adhereed to the glaſs; the latter circumſtance was owing to the ſpark having melted alſo the inner ſuperficies of the glaſs, as might be diſcovered with a lens, or even without ſuch aſſiſtance (*Atmoſ. Terr. Elect.* p. 254.) II. Scrapings from *laminous* jet became calcinated; the few parts of them through which the ſpark had paſſed, could imbibe water, and were thereby turned into a kind of mud. III. Litharge of lead acquired a yellowiſh tranſparent colour, like that of amber, which is the colour of litharge when vitrified. IV. From verdegreaſe there conſtantly reſulted a ſpot of a vivid deep red colour, and as gloſſy as poliſhed marble, which I immediately acknowledged to be *glaſs* of copper. V. From calx of antimony, I obtained a ſpot of ſuch a yellow colour, and ſuch a tranſparency, as made me acknowledge it to be *glaſs* of antimony. VI. A little powder of zinc, at the inſtant that it was traverſed by the ſpark, threw two ſmall flaſhes of whitiſh ſmoke, out of both ends of the tube, which furred ſo much the

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inner furface of it, that *flowers* of zinc were very eafily difcernible on it.

740. With refpect to *metallifying* again metallic *calces*, it appears, that fparks do not effect it otherwife than by driving into the lateral furfaces of thofe *calces*, what remaining phlogiftic they may find in thofe parts of them, which they immediately enter. It is true, that the fmoky track effected by the paffage of a fpark through a bit of cinnabar, is all over fprinkled with drops of mercury, and I have feen fuch drops alfo arife from feveral other mercurial compounds; but thefe drops, we are to obferve, were not *revivified*, they were merely made to unite again together; for the mercury, in all the above compounds, retains every requifite fubftance or principle to make it exift as mercury, only they are divided into fuch fmall particles as render them unperceivable to our fenfes. I have repeated feveral of the experiments above, by fending the difcharges of three large cryftal jars through the feveral fubftances above mentioned, after inclofing them between cakes of wax; and the effects were exactly the fame with thofe before, only fomewhat more intenfe.

741. The following experiment may, I think, ferve to fhew how fparks can turn metallic bodies into real metal, by driving and carrying to them fuch phlogiftic as they may raife from the places of their paffages, and imprinting it into the furface of thofe metallic fubftances. With a little oil, anoint fome fmall fpot on the furface of a well polifhed plate of iron, and fend a fpark through that fpot, you fhall find its colour turned into an almoft argentine colour, like that of the fineft fteel; which, I think, is owing to the phlogiftic which has been added to the iron. The fame will obtain if wax, or tallow, are ufed inftead of oil; without fome fuch like ointment, fparks can produce no fuch effects.

CHAP.

CHAP. V.

On the electric fire, relative to common fire.

742. IN examining all the above effects of electric ſparks, with reſpect to air, to water, to living bodies, and to foſſils, we may perceive that they are ſimilar to thoſe produced by common fire; and all the difference between them may be reduced to the following, viz. that common fire acts with great ſlowneſs, when compared with the electric fire, and with leſs efficacy; which, perhaps, is owing both to its being leſs denſe than the former, and being mixed with inactive ſubſtances.

743. This ſimilarity is rendered ſtill more obvious, by conſidering how eaſily either ſpirit of wine, the ſmoke of a candle juſt put out, powder of colophony, or gunpowder, and ſuch like inflammable bodies, may be kindled even by very ſmall ſparks, in conſequence of their phlogiſtic being very copious, and litte fettered by either acqueous, ſaline, or terreſtrial particles. The leaſt imaginable ſpark will inflame æther; ordinary ſparks ſent through a fur of rectified ſpirits of wine, a little warmed if the experiment be made in winter, will kindle it; even ſparks drawn from a piece of ice will light a candle juſt extinguiſhed. A ſtrong ſpark from the Chain merely, will kindle the ſmoke of colophony; but, in order to inflame bare colophony reduced into powder, the ſtrongeſt ſpark from a coated plate muſt be ſent through it; to that end, the powder muſt be ſpread on the coating of the plate, and a little warmed if the experiment be made in winter, and the ſparks muſt be drawn through this powder; with regard to gunpowder, it is neceſſary, in order to kindle it, to ram in and carefully compreſs its particles, in order the more to condenſate the ſpark between them.

744. With regard to other bodies leſs inflammable than thoſe above, there is this analogy between them, that the electric fire kindles pretty eaſily thoſe in which it finds a ſufficient quantity of phlogiſtic, or which ſtand near enough to bodies replete with

that ſubſtance, to enable the electric fire to drive a certain quantity of it into them. Place a leaf of paper, anointed with oil, wax, or tallow, in the middle between two quires of paper, and under the whole, place a ſheet of tinfoil; then ſend a large ſpark, ſuch as that from my three large cryſtal jars, and you ſhall find that both the anointed leaf of paper, and the two next, have been burnt around the hole made in them by the ſpark; you ſhall even find the edge of the hole in the laſt leaf, which is contiguous to the ſheet of tinfoil, blackened a little; and if you examine this latter hole with a lens, you ſhall find it almoſt filled with particles of melted tin; with regard to the leaves in the middle of the quire, they preſerve their uſual colour. It appears that the rapid ſpark can diſengage a certain quantity of phlogiſtic, only where it finds it in a certain degree of copiouſneſs; it ſeems alſo that the ſpark avails itſelf much of the phlogiſtic it has thus exploded, for kindling ſuch bodies as ſtand near, though leſs replete with it; but of all this I ſhall ſpeak more fully, when I come to treat of lightning, as it will afford us much more extenſive and expreſſive examples. At the *Superga*, the ſilk ſtrings which keep the wires ſeparated, are often ſinged by the burning flaſhes of phlogiſtic, which the ſtrong ſparks that are thrown from the neighbouring extremities of theſe wires drive againſt them.

745. Another fine analogy between the electric and the common fire, I alſo diſcover in the rings of priſmatic colours which they equally produce, and which Dr. Prieſtley only conſiders as particular effects of electric ſparks. The doctor has deſcribed theſe rings in the fifty-eighth volume of the Phil. Tranſ. I receive only now that volume, with the following (the fifty-ninth), and, along with them, the Memoir concerning the above *rings*. I find two others, beſides the above; the one treats of the lateral ſtrength of electric exploſions, and the ſubſtance of it is the ſame with what I ſaid in p. 77 of *Atmoſ. Terr. Elect.* with reſpect to the lateral exploſion of air. In the other Memoir, I find that Dr. Prieſtley had diſcovered, before me, the peculiar reſiſtance which a diſcharge meets, when obliged to circulate through a long ſpace, within deferent ſubſtances. It was a fine experiment which he made, by uſing a pair of tongs, inſtead of a conducting

ducting bow, for difcharging a plate of glafs: when the two branches of the tongs had been brought pretty near to each other, he obferved, that a part of the difcharge, inftead of running through the long circuit of thofe two branches, preferred to leap from the one to the other, through the air.

746. The fubftance of the other Memoir, of which I am now more particularly to fpeak, may be reduced to this point, viz. that a ftrong fpark from three glafs jars, which is made to rufh from a plate of metal into the point of a needle perpendicularly placed upon it, leaves around that part of the plate through which it paffes, and which it deeply melts, feveral very thin concentric circular rings, imprinted on the metals, which are like the colours of the rainbow. From this phenomenon I derive another analogy between the electric and the common fire. I light a few fmall coals on a forge, exactly under the funnel; upon thefe charcoals I place a thin, long, and polifhed fpring of fteel, which is folded; then examining it attentively around the center of the action of the heat, I fee the fame colours take place as thofe above; only, the bands which form fuch appearances, are the more tortuous and broad, in proportion as the heat of the coals proceeds in the prefent cafe from a greater number of parts difpofed with lefs regularity.

747. Univerfally, the heat produced by electric fparks, or by common fire, extends over a blade of fteel, with an intenfity which leffens, in proportion as the diftance from the principle of fuch heat increafes; and thefe two fpecies of heat thus reduce the fuperficial parts of the blade to the refpective degrees of tenuity which are requifite, that they may, according to the calculation of the great Sir Ifaac Newton, reflect rays of various colours; we are, therefore, by thefe experiments brought acquainted with the exiftence of a fimple geometrical regularity obtaining in the operations of heat, in which our fenfes had hitherto allowed us only to perceive a kind of perturbated motion.

748. Having placed a thin, polifhed, ample plate of brafs, and another of filver, on charcoals, I faw on them the fame feries of colours as above. I. The experiment of Dr. Prieftley, who, by repeatedly fending frefh fparks, widened the internal rings

of colours, is not inconfiftent with the explanation above given; new fparks bring more diftant particles of the plate to a ftate of greater tenuity than before, and the effect is in this cafe the fame as that which is fo remarkable in bubbles made with foap-water, the upper part of which grows continually thinner, in confequence of the weight of the water, and the coloured circles formed by it become gradually broader. II. The circumftance of the rings being rendered ampler when the point of the needle remains a little diftant from the plate, is confiftent with the other effects of fparks; as thefe fparks leap farther from the point, they muft needs fpread themfelves more, in the fame manner as electric brufhes do.

749. But of all analogies between the electric and common fires, the moft remarkable is that of the manner in which they propagate themfelves through different mediums, or bodies; in regard to this, the following rule obtains, viz. *the common fire propagates itfelf with moft difficulty, through fuch bodies as refufe to conduct the electric fire, and moft eafily through thofe which can conduct it.* Dr. Franklin (pag. 259.) firft mentioned this analogy in a letter which he fent to the Royal Society, in the year 1756; I fhall here beftow a few words on the fame fubject. I. *The common as well as the electric fire, are fpeedily diffipated in dilated air.* The following experiment will ferve to fhew the truth both of this fact, and of the reafon affigned in 277, why a flame is quickly extinguifhed in dilated air. I hold a round fpoon, an inch and an half wide, under the beak of a lamp, and having lighted fome paper placed upon it, the flame of the lamp is extinguifhed; the reafon of this is, that the ample flame of the paper drives the air from the fpace that furrounds the flame of the lamp, whence the whole fire of the latter is invifibly diffipated, and forfakes the wick. The fpontaneous flafhes of fire emitted by the phofphorus of Kunkel under the glafs bell of an air pump, which is gradually confumed, proportionally to the copioufnefs of thefe flafhes, is very conformable to the explanation juft given; the facility with which the copious fire contained in this phofphorus can of itfelf efcape (which makes it neceffary to keep it conftantly dipped in water); enables it ftill more eafily to fly and burn away, when

placed

placed in a vacuum; now, the flame of a burning candle is diffipated exactly in the fame manner; only, the greater adhefion of the fire to the fubftance, which is the pabulum of this flame, is the caufe that one flafh being once diffipated, another does not fucceed.

751. *Common fire, and common heat, are, as well as electric fire, retained proportionally longer, in denfer air.* A certain quantity of air laterally driven againft a flame, feparates it from its pabulum, fo that it can excite no new fire, or flame, from it: but when air is driven directly againft the ignited body, it carries the fire which is difengaged out of the fame, againft the aqueous, faline, or terreftrial particles of which it is compofed; it thereby again difengages the fire which lies inclofed within them, and thus animates the flame. If we raife our thoughts to more extenfive contemplations, we fhall be likewife convinced of the infinite goodnefs and wifdom of Providence, that has immerfed us in a medium like air, which fo efficacioufly repercuffes and preferves our vital heat. Is it not another effect of the fame infinite wifdom, that heat being only preferved in a fmall degree on the tops of mountains, in confequence of the rarity of air, rains fall there not only with great plenty, but alfo concreted under the fhape of fnow; fo that they become, as it were, coacervated to a great height, and, flowing from thence with flownefs, perpetually maintain the refervoirs which form rivers, that is to fay, the fprings of the fertility, as well as the induftry, of the populous plains.

752. II. *Common fire like the electric fire is eafily diffipated through moift vapours.* The only reafon, I think, why a candle placed above the furface of boiling water, or of grapes, which are ftrongly fermenting, foon goes out, is becaufe that portion of fire which is already difengaged from the *pabulum,* can then freely efcape through the moift vapour raifed from the grapes, or from the water; it was by vapours of this kind, plentifully diffufed in an open fpace, and continually fucceeding each other, that the flame in numb. 277, was extinguifhed. If we proceed to experiment on permanent inclofed vapours, we fhall find, that the analogy between electric and common fire ftill continues to obtain; nay, it proceeds farther, and likewife takes place in phenomena of a contrary

contrary kind to thofe above mentioned; that is to fay, *as thofe bodies which can conduct the electric fire, yet refufe to tranfmit more than a certain quantity of it through their own fubftance, fo inclofed permanent vapours, impregnated with a certain quantity of common fire, which they cannot diffipate by tranfmitting it to other bodies, will fuffocate a flame.* To prove this, you may ufe the fimple experiment of an extinguifher of paper, made by properly fixing a piece of paper to the ring of a large key. Put this extinguifher over the flame of a candle, and as you lower it, the flame will fhorten itfelf, without penetrating into the paper; indeed the latter will be half burnt and blackened by the hot effluvium, but will not be kindled. The flame cannot diffufe itfelf away, through the vapour contained in the extinguifher, as it would do in the open air; and this vapour being once replete with fire, fuffocates the flame, by refufing and repercuffing it: to be fhort, the vapour inclofed and confined in the extinguifher, confines the flame, in the fame manner as the particles of this fame vapour, before their being difengaged from the pabulum, imprifoned the fire in it, and fixed it.

753. I prick the extinguifher with a needle, when a fmoke, but no flame, rufhes out of it: this is becaufe the vapour which continues to be ftopped, ftill poffeffes a greater efficiency for fuffocating the flame, than for animating the fmall part of it that is now allowed to depart: this is likewife the reafon for which, from too fmall intervals between pieces of burning wood, no flame, but only fmoke, can fpring out. Out of thefe fuffocated inclofed vapours, that from gunpowder muft be excepted; the ftrength with which its flame propagates itfelf, which is owing to the affinity between charcoal and nitre, exceeds, at leaft to a certain degree, the fuffocating force of the vapour, and the latter, which fpreads itfelf as ufual, with a very great expanfive force, fpreads itfelf (owing to the faid affinity) actually kindled and inflamed.

754. Let us now return to our fubject. In order to prove the common property of electrical and common fire, of being diffipated by moift vapours, this obfervation furely fuffices, that all bodies, in general grow cool, in proportion as a greater quantity of moifture emanates from them. Whence it happens, that by wetting a ther-

a thermometer expofed to a current of air, with any kind of liquor, the mercury in it lowers in proportion to the quantity of moifture which evaporates from it. Thence alfo the experiment made by Dr. Franklin, who, wetting the ball of a thermometer with æther, while another perfon blew againft it with a pair of bellows, faw the mercury in it lowered feveral degrees under the degree of congelation. In fummer, I place a ftratum of ice fix or eight lines thick, on a table which is unpolifhed. I leave it till it begins to melt, then I cover the upper furface of it with falt, and while this upper furface melts and gives a fmoke, the under furface, which at firft thawed, now begins to freeze again, and fticks to the table. This is to be explained by confidering, that the vapour emitted from the upper furface of the ice, ferves to carry the remaining heat of the under furface, in the fame manner as the vapour which rofe from the ball of the thermometer above mentioned, diffipates the heat in it. Thence proceeds the coolnefs of water contained in veffels, either wrapped in wetted cloths, or made of clay which has not been varnifhed, and through which the water continually oozes. Whence the coolnefs of the leaves of trees, if not from the moifture which continually perfpires from them? Whence the coolnefs of meadows, if not from the continually emanating vapours, which, befides the heat of the grafs itfelf, carry away fucceffive portions of that which lies diffufed in the ambient air.

755. Our vital function confifts, in a great meafure, in difclofing fire from our aliments; which is effected by triturating them, by mixing them together, by making them circulate, and by diffufing into them the heat already diffufed in former juices, in order to difclofe that which they contain. It is neceffary that this heat, which perpetually reproduces, or excites itfelf, in the fame manner as the flame of a candle does, fhould perpetually emanate from our bodies, in a certain quantity, exactly as the flame of the candle requires, in order to its continuing to live, to be continually diffufed away; now aqueous liquids are, in animal bodies, the chief conductors of this heat. Thus it is, that harveft-men preferve themfelves from the deadly effects of the external heat, which would otherwife keep within them their own internal heat, and in-

creafe

creafe it to too high a degree, by plentifully drinking and perfpiring; thus, we can by the help of a fan, cool ourfelves, by bringing continually new dry air into contact with our face, which may carry away moift vapours, and, confequently, the heat from it. The feaman, fays Dr. Franklin, in his letter XXVI. in which this great Obferver treats this fubject with his wonted fagacity, the feaman puts his finger into his mouth, and from that part of it which he feels to grow cool fooneft, judges from whence the wind is to blow. If fuch emanation of vapour from our bodies be too copious, then our vital flame is too much diffipated; it is thus we feel a painful coldnefs, when we get out of water after bathing; the moift fur which covers our body, by being diffipated, carries away too much of that heat, which I may call *innate* with us, fince we bring it into the world from the maternal womb, along with thofe vital functions, by the help of which we afterwards preferve it.

756. Cannot we, in the fame manner, impute all kind of artificial cold, efpecially thofe produced by chemical mixtures, to a diffipation of heat, caufed by a diffipation of fome vapour, in thofe mixtures, which is apt to conduct common fire away with great efficiency? This fagacious extenfive conjecture alfo belongs to Dr. Franklin; and, befides, agrees with the method defcribed by Mr. Mufchenbroeck, in his Commentaries on the academy *del Cimento*, for lowering the mercury of a thermometer; which is by dipping it into a number of flakes of fnow, which are fucceffively deftroyed by means of fuming fpirit of nitre. The latter is the method by which they have, at Peterfbourg, fucceeded to coagulate mercury; though, I muft add, that I think that as Nature hath two ways of cooling bodies (the one by diffipating their fire, the other by fixing it) fo, we muft allow to Art alfo, two different ways of producing cold. When the acid of vitriol, which is difclofed by ruft, from a ferrugineous body, is mixed with the phlogiftic of powdered charcoal, only an external tranfient heat is manifefted, but the greater part of the fire of this powdered charcoal remains fixed, as I think, in the fmall mafs of *fulphur*, which refults from the mixture.

757. III. *Does not common fire propagate itfelf more eafily through metallic bodies, than through other fuch bodies as reprefs or refufe electrical*

electrical fire? This is evident, from the much greater facility with which water boils in veſſels of iron, of braſs, or of tin, than in thoſe made of varniſhed clay; which obſervation may ſerve to throw a light on the uſeful art of keeping heat more united, more intenſe, and preſerving it longer, &c. Dr. Franklin poured melted lead into a kind of ſquare box, the bottom, and three ſides of which, were of wood, and the other ſide of lead; and, he obſerved, that the latter ſide was ſooner heated through, and cauſed a quicker coagulation of the melted metal than the others.

758. This analogy may be carried much farther; *heat may render ſuch bodies conductors of the electrical fire, as would otherwiſe refuſe to conduct it*; *and cannot we alſo think, that the quantity of common heat already introduced into a body, enables it to receive and transfuſe a ſtill greater quantity of it?* Thus, the heat of a lighted candle, by gradually penetrating into the wick, excites another effluvium from it, which is ready to kindle, and fly away in a vertical direction. It is the heat of the ſmoke ariſing from a candle juſt put out, that cauſes it, when received in a tube of paper eight inches long, or more, to kindle, as ſoon as a lighted candle is brought near the upper orifice of the tube; when the flame of the latter ſeems to precipitate itſelf towards the wick of the former.

759. *Is not, moreover, the property of electrical fire of diſpoſing deferent vapours into its way, likewiſe proper to common fire,* beſides their noted common propriety, of always *diffuſing themſelves to an equality?* It is, no doubt, the common fire, that, by diffuſing itſelf to an equality from the ambient air towards thoſe bottles or jars which are brought cool from the cellar, carries to them the deferent moiſture which we find to coat them. It is alſo common fire, which, in winter, by diffuſing itſelf from a warm room, towards the cold panes of glaſs in the window, carries to them the deferent moiſture, which, by loſing its fire cloſe to them, begins to freeze. It is the ſubterraneous heat which, either the rivers, or the ſea, continually draw from their beds, which diffuſes temperate deferent miſts into the ambient air. The following is a fact which I conſtantly find to take place: whenever, in winter time, the miſt on the river Po, which is diſtant only one thouſand four hundred paces from

my habitation, reaches to the height of a certain window on the north ſide of my apartment, the mercury of the thermometer which I keep at that window, is ſuddenly lowered one or two degrees; which is owing to the heat which diffuſes itſelf from the river, and carries this ſlow miſt along with it; in the ſame manner as, in a cold morning, the exceſs of the ſubterraneous heat raiſes a very conſpicuous deferent vapour from the bottom of wells.

CHAP. IV.

On electric ſparks relative to light.

760. COMMON fire, when diſplayed over the ſurface of inflammable bodies, aſſumes colours which are extremely varied, according to the nature, whether aqueous, terreſtrial, or ſaline, of the ſubſtances by which it was impriſoned, and with the effluvia of which it always, though being in an active ſtate, remains more or leſs mixed. As to electrical fire, it is ſo much leſs adherent than common fire to the ſubſtance of bodies, that it commonly manifeſts itſelf free from any mixture of them; nor does it ſeem that it exhibits any other difference in its appearances, than what may reſult from its different degrees of denſity.

761. *However, ought not the deferent vapours that conduct intenſe ſparks along with them, produce ſome change in the appearance of the latter?* Indeed, to this cauſe, I think, we are to impute, at leaſt in ſome degree, the remarkably red colour, aſſumed by the ſpark which Dr. Prieſtley ſent along the ſurface of a veſſel full of oil of vitriol. The reddiſh colour of ſparks, ſent into animal bodies, is, very probably, likewiſe owing to the vapour which they raiſe, either from feathers, or from hair; the ſinging of the latter, as well as the ſmell ariſing from it, furniſhes a ſufficient proof of the exiſtence of ſuch vapours. The ſpark which imprinted metallic particles, into the above mentioned cylinder of glaſs (700) ſeems to have been loaded with a great quantity of ſuch particles. Now, muſt not the outward appearance of this ſpark have been

greatly

greatly altered, in confequence of the ftrange fubftances which, as it were, vefted it? indeed, accounts are very frequent of lightning that has been propagated almoft invifibly, through extenfive tracts of very thick clouds.

762. If we except the cafes in which the appearance of the light of the electric fire, is altered by fuch accidents as thofe above mentioned, we may fafely fay, that this fire is of itfelf extremely pure; and I never could underftand how it might come into the head of fome philofophers, to think that the light of electric fparks does not contain the feven prifmatic colours. Do we not fee objects coloured and difpofed in the fame manner, by the light of electric fparks, as by that of a candle, or of the fun? and if any doubt on that fubject remained, the infpection of fuch fparks through a prifm, would foon remove it. The firft Obfervers who thought of making an experiment on that fubject, did not perhaps take care to direct their eye towards that place where the fpark appears to be, when looked at through the prifm, which is very different from the place where it really paffes; and others have, perhaps, made hafte to copy the obfervation, merely becaufe it was an extraordinary one.

763. The light of electric fparks, like every other kind of light, muft naturally have the property of raifing a *phofphoreity*, that is a power of giving light in the dark, on the furfaee of feveral bodies; yet it is only chance that has difcovered this property to M. de Lane, fo flow our reafonings fometimes are. Dr. Franklin gave me the firft information of this difcovery; and, at the fame time, fent me fome of the excellent phofphorus difcovered by Mr. Canton, informing me, that it continued to emit light for a confiderable time, when the flafh of a fpark had been excited near it. I thereupon betook myfelf to make experiments on the numerous clafs of phofphoreous bodies, enumerated by my refpected friend, Signor Beccari, in the *Comentari Bolognefi*, and was foon enabled to anfwer Dr. Franklin, that the *phofphoreity* of the electric light was the fame with that of the folar light. I fhall relate here fome of the experiments of which I wrote him the defcription. A full difcharge of my ufual coated plate, fent through powdered fugar, produced the beautiful light of a copious luminous fhower, and

left, as it were, a *glory** of light around the place which drew the ſparks. I alſo found that a like *glory* remained imprinted on the ſurface of a very fine dry linen cloth. Thoſe bodies which Signor Beccari has obſerved to retain ſolar light in the higheſt degree, I have obſerved likewiſe to retain beſt the light of electric ſparks.

764. I made alſo, in this ſame letter, if I remember right, a digreſſion on the light emitted by ſalts while they are pounding. Sugar, for inſtance, while it is pounding, emits light more eaſily than any other ſalt, as well as more plentifully, and more like that of electric ſparks. In order to obſerve the light yielded by gem ſalt, rock-alum, and by other ſalts in general, the eye muſt be well prepared, and intirely free, to uſe a phraſe agreeable to the ſubject, from any phoſphoreity of its own; and ſuch ſalts muſt, beſides, be pounded with blows that rapidly ſucceed each other. You may, when in the dark, frighten ſimple people only by chewing lumps of ſugar, and, in the meanwhile, keeping your mouth open; which will appear to them as if full of fire: to this add, that the light from ſugar is the more copious in proportion as the ſugar is purer. By breaking in the dark the outward cruſt of extremely refined ſugar, which covers certain comfits, a light ſprings out of it, which is exactly like that of electric ſparks. I muſt, however, confeſs, that notwithſtanding the ſimilarity of the outward accidents, I could not ſucceed to aſcertain whether the ſubſtance which lightened, while ſalts were pounding or breaking is the ſame with the light of the electric fire: very thin threads applied both within and without, to the ſides of a glaſs mortar in which I pounded refined ſugar with a glaſs peſtle, produced no ſign whatever of electricity; neither did threads placed cloſe to the ſugar itſelf, or affixed to the inſulated tongs with which I ground it, afford any electric ſign.

* A *glory*, is a word uſed in Catholic countries, to expreſs that luminous ſplendid appearance, which is ſuppoſed to ſurround the heads of ſaints, of the Virgin Mary, or of God himſelf, and with which they are repreſented in the pictures that are made of them; hence the metaphorical expreſſion, a *glory* of light, has been formed, which is frequently uſed in common ſpeech and writing, and is meant to ſignify any kind of extenſive luminous appearance.

765. This

765. This light yielded by ſalts muſt, therefore, proceed from a ſubſtance which finds itſelf in much the ſame ſtate within them, as common fire is within bodies in general; and the violent ſeparation of the particles of thoſe ſalts, vibrates ſuch ſubſtance through very ſhort intervals, and without intirely freeing it out of the pores in which it is contained. But are not ſalts the chief links uſed by nature for confining and fixing the ſubſtance of fire? What are pyrites in the mineral kingdom, but ſalts, which, being united with a few metallic particles, confine a great quantity of fire? and in the vegetable kingdom, does not common fire diſengage itſelf the more eaſily from woods, in proportion as the latter are more free from ſaline particles; whether they are naturally ſo, or a diſſolving moiſture has rendered them ſuch. Why is the flame which riſes from beech-wood that has been ſent down rivers in floats, ſo mild and clear, if it is not becauſe water has diſſolved the ſalts which the wood contained? Why does wood decayed by lying during a long time in moiſture, emit light; and why is it, at the ſame time, much lighter and brittler than common wood, if it is not becauſe ſuch moiſture has diſſolved and carried away thoſe heavy tenacious links, ſalts? In conſequence of this diſſolution, fire is become enabled to diſengage itſelf by virtue of its expanſive force, from its now widened cells, and to begin to move, though indeed with ſuch ſlowneſs, and in ſuch ſmall quantity, as only allow it to yield light, but not to afford any heat that may be perceived by our ſenſes. Is not the above, the true reaſon of the light produced by cockle-fiſh and ſea-fiſh, and of that which appears, in dark nights, on the ſurface of the ſea, and is cauſed, as Mr. Canton has ſhewn, by floating ſubſtances that putrefy.

766. One thing I have ſucceeded to aſcertain with reſpect to another kind of light, which, as far as I know, has not been obſerved by other experiment-makers, and is, as I have found, a real electric light. *I mean the light emitted by glaſs, and other inſulating bodies of a certain thinneſs, when violently ſtruck by air.* Here follows the ſubſtance of what, on a former occaſion, I wrote with regard to the diſcovery and nature of ſuch light, to my illuſtrious friend the Count of Scarnafigi.

767. I.

767. I. Wifhing to difcover fomething with refpect to the electric fire, with which the fubftance of our air is replete, I had formerly attempted to fee whether, while I drew the air from a glafs bell, in the dark, I could perceive any light, or whether I could perceive any motion in the hair-threads which I had fixed to the faid bell; but my endeavours proved ineffectual.

768. II. Sufpecting that the fmall electricity, which I fuppofed to be produced by the dilatation of the air, might remain unperceived, on account of the flownefs of this dilatation, and imagining no poffible means of fuddenly producing a vacuum, I thought of breaking in the dark a few glafs balls that had been blown, when I faw an inftantaneous light take place, the vividity and amplenefs of which was proportioned, it feemed, to the amplenefs of the ball, and likewife to the noife which the air made, when fuddenly filling the vacuum that was opened to it. This light was much like the mild light which is diffufed by thofe flafhes of lightning which take place in ftormy weather, and extend beyond the limits of the clouds, without offending the eye.

769. III. In breaking thefe balls, a fmoky fmell takes place, which moft probably proceeds from the green rufhes which are put in thofe balls, when they are made; in order that the vapour from them may fwell the balls. Defiring to afcertain whether the light which appeared when the balls were broken, did not alfo proceed from a certain quantity of fire that was difengaged from this fame fmoke, I procured balls blown with a lamp, and they alfo emitted light, in a lefs degree indeed than the former, but very vifibly, efpecially when I broke them within a large cup of white clay, and took care not to fhut my eyes at the inftant that I ftruck the ball. The decreafe of the intenfity of the light in the latter cafe, was proportioned to the diminution of the exactnefs of the vacuum, and of the bulk of the balls; thefe two circumftances caufed the explofion to be effected with much lefs force in the latter, than in the former inftances.

770. In order to afcertain ftill farther the truth of the above reafoning, I narrowed, with the help of a lamp, the neck of fome

ſome Florence flaſks; I introduced ſome water into them, which I made boil, and then ſuddenly cloſed their orifices; this done, I found, when I broke them, that they emitted in the dark as much light at leaſt as the balls that had been blown at the furnace; therefore, the light of theſe balls does not ariſe from any ſmoke contained in them, and the cauſe above aſſigned to the diminution of the light that took place when only balls made with a lamp were uſed, was the true one.

771. In order ſtill more to free the above light from any ſuſpicion of a mixture of common fire, I drew the air from ſeveral large flaſks with an air-pump, and they all gave me a light when I broke them; which light was proportioned, as to its extent and vividity, to the bigneſs of the flaſk, and the loudneſs of the exploſion that had taken place. This proportion I have found pretty conſtant in all my experiments. Such balls as were cracked and ſomewhat full of air when I broke them, afforded no light, as well as no noiſe; others which received only a crack from their fall, imbibed air ſlowly, and did not lighten; other balls, or flaſks, only imperfectly emptied of air at firſt, produced both a ſmall light, and a ſmall noiſe.

772. Care muſt be had, when larger flaſks are broken, leſt ſhivers from them ſhould leap to the face, and endanger the eyes of the obſerver. When I experiment with thoſe flaſks, I uſe to look through a plate of cryſtal; and in order to preſerve it from being obſcured by my breathing, I adapt to my noſe and mouth a ſheet of paper which protects it.

773. In order to diſcover whether the light I mention proceeded from the ſtroke of air, or whether it ſimply aroſe from the breaking of the glaſs, I took to experiment with a few of the celebrated pieces of glaſs, ſometimes called *Batavic drops*, which, as I had obſerved before, equally break in the vacuum, and in air; and by their means I procured inſtances of impetuous fractures of glaſs, made without any ſtroke from air. I therefore broke a large quantity of them, and neither I, nor any other perſon, ever could perceive in the completeſt darkneſs we could produce, any light ariſing from them. M. Poliniere ſays, he once ſaw a ſmall light, which begun at the place of the breaking, and ran up to the head of the drop.

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The Abbé Nollet fays, that he thinks he remembers his having once feen fome fuch light; but, to me, it appears, that M. Poliniere faw too much, and that the Abbé was fatisfied with too little.

774. In thofe Batavic drops, however, a few fmall bubbles are to be difcerned: thefe gave me fome miftruft, left they fhould be filled by air, and I tried again the experiment upon *Bologna bottles*, the thick bottom of which was without any fuch bubble; their fhivers, when they broke, were driven away with great force, but I could not perceive the leaft light from them. One indeed of thefe bottles emitted a light, but this circumftance was joined with that of a pretty loud crack, and the bottle was, as I found, both a Bologna bottle, and a *ball* emptied of air at the fame time, like thofe above mentioned: I had hermetically fealed it at the furnace, and fetting apart the thicknefs of its bottom, it was much like thofe flafks into which, a few years before, I had feen the noble and fagacious Count of Scarnafigi, put fhavings of tin, which he afterwards kept for a long time in a ftate of fufion, with the flame of a lamp of fpirit of wine, in order to fhow that they received thence no increafe of their former weight.

775. I broke again feveral Batavic drops, in the vacuum of the air-pump, and could not perceive the leaft light.

776. Having thus afcertained, as far as I poffibly could, that the light perceived in breaking glafs *bombs* and flafks, arofe only from the ftroke of the air, I undertook to examine whether fuch a ftroke againft any other fubftance would produce the like effect. I, at feveral times tied bladders on the orifice of a thick drum of cryftal, which I ufed as a glafs bell, and when the outward column of air broke through the bladder, and precipitated itfelf through it, I could not perceive any light. I very often by ufing thick ftrong bladders, fucceeded to bring the vacuum within fuch drums to a great degree of exactnefs, and at other times, I broke the bladder with my finger, when only ten, eight, five, or only four ftrokes of the embolus had been given, and I never could perceive the leaft appearance of light; whence I concluded, that the light perceived when glafs bombs and flafks were broke

did

did not proceed from the ftroke of the outward air againft a remnant of inward air.

777. I now placed under the drum a very thin flafk of glafs, and when the air fuddenly broke through the bladder fkin, I had the pleafure to fee fmall flafhes of lightning arife in the place where I knew the flafk lay. I afterwards fubftituted flafks of thick glafs, and the ftroke of the air againft them did not produce any light. I alfo placed feveral veffels, or plates, of different metals, and of different fizes and fhapes, within the drum, and no light ever refulted from the ftroke. I afterwards placed under the drum a cylindric veffel of fealing-wax, an inch wide, three inches high, and pretty thin, and at every time that I made the experiment, I faw it lighten, efpecially around the brim, which was thinner than the body of it, and fomewhat uneven; this circumftance I fhall add, that none of the ftrokes of air, though ever fo vehement, ever happened to break the veffel; which was owing, I think, to the equality and fimultaneoufnefs of the ftroke, on both the internal and external parts of it.

778. Thus, through a feries of experiments, I afcertained this fmall truth, viz. *that the light emitted by glafs bombs and flafks emptied of air, is an electrical light, which the impetuous ftroke, or vehement friction of the air, excites from infulating bodies*; this particular fact does not indeed difcover any new relation between light and the electric fire; but it affords a new proof of the fact generally obferved, viz. that the electric fire emits light whenever it is made to flow between infulating bodies, along their furfaces.

779. It might be here the proper place to examine this interefting queftion, how it happens, on the one hand, that electric fire cannot pafs through infulating fubftances, and on the other, that *electric* light fo freely pervades them. In fact, the fpark which is thrown in the infide of a glafs veffel, is feen from all fides: even through common wax, or fealing-wax, you may fee the fparks excited on their other furface. In the firft chapters of this work, I confidered this fact as very fingular; though, in truth, it is only analogous to what likewife happens in the above mentioned fubftances, with refpect to common light. If you look

underneath the edge of the top of a lighted candle, whether it be of wax or of tallow, you ſhall ſee it to be penetrated by the light of the flame. Adjuſt ſtrata of the ſame ſubſtances to a hole made in a well cloſed window, and which is the only paſſage for light into the room, and you ſhall ſee the room lighted to a certain degree, by the light that will penetrate through the above ſubſtances. In order to explain theſe facts, the common hypotheſis might ſuffice, that light propagates itſelf through the preſſion of a medium which is univerſally diffuſed; and in this caſe we might likewiſe conceive this medium as being alſo vibrated within inſulating ſubſtances. But to this hypotheſis, the conſiderations offered by the ſagacious Sir Iſaac Newton, as well as the experiments made by Mr. Canton, are contrary. The latter gentleman has, in the Philoſophical Tranſactions, in the treatiſe in which he gives a receipt for eaſily making excellent phoſphorus of Bologna, has, I ſay, ſhewn how to make ſuch phoſphorus preſerve for ever its virtue; which is by hermetically ſealing it within glaſs very carefully dried; and he proves, that this phoſphorus, partly of itſelf, partly by being heated, gives back what light it has received, and no more. To the above hypotheſis are alſo contrary the experiments I have made, which I mentioned laſt year to Mr. Canton, and have more fully deſcribed to Father Boſcovich: by theſe experiments I proved farther, that the ſame phoſphorus gives back the light it has imbibed, juſt ſuch as it received it; that is, of a ſame hue as that of the different cryſtals through which it received it. We may, therefore ſay, in order to explain the queſtion propoſed above, that the light emitted by an electric ſpark, conſiſts of particles incredibly more ſubtle than the ſpark itſelf, and that the quantity of its ſubſtance is almoſt nothing, when compared to that which forms the body of the ſpark. The better light enables us to perceive other material ſubſtances, the leſs our ſenſes are able to penetrate into the ſubſtance of light itſelf; and indeed it ſeems that the analogy fails between its *materiality*, and the *materiality* of other ſubſtances.

SECTION

SECTION V.

On the electric tickling and wind; on the bruſh and the ſtar.

CHAP. I.

On the electric tickling and wind.

780. IT ſeems to me that the electric wind has not been ſufficiently diſtinguiſhed from another impreſſion like that of a *tickling*, which the electric fire excites in us; nor do I know that any juſt cauſe of it has hitherto been aſſigned. I. When, in very proper electric weather, you immerſe your face, or the back of your hand, into the briſk electric atmoſphere of a ſufficiently ample body, whether inſulating or deferent, you feel a kind of tickling on your ſkin, the ſame as if you moved through cobwebs; that is to ſay, the exceſſive fire which actuates the above atmoſphere, drives the natural fire from the contiguous air to your face, and this fire, in getting into your ſkin, produces the above ſenſation of tickling: if the body near which you ſtand, be electrified by deficiency, then the fire within your ſkin flows to the external ſurface of it, in order to accumulate itſelf on it, and form an exceſs; now, this fire produces the ſame kind of feeling, whether it iſſues out of your body, or gets into it. II. Hence it comes, that when you are immerſed in a given part of a quiet electric atmoſphere, you feel nothing; but in the very inſtant that, by extracting a ſpark, this atmoſphere becomes annihilated, you feel the above ſenſation of tickling renewed, in conſequence of the fire which now iſſues out of you, or gets in. III. If you bring your face ſo near the electrified body, that the fire may paſs divided from the one to the other, the tickling will grow ſtronger,

 and

and more pricking, whenever the fire will begin to be ſomewhat more united, and will want only one degree more of union to form real ſparks. Therefore, we may diſtinguiſh two kinds of tickling; the one ariſing from a ſimple atmoſphere, the other from an actual electricity, which diffuſes itſelf in a divided ſtate. Upon the whole, we may lay this principle down, that *ſenſations of an electrical tickling will ariſe from the paſſage of a certain quantity of electric fire into your ſkin, or out of it*; *and that this ſenſation will be weaker, in proportion as the diviſion of ſuch fire will be greater.*

781. From the above tickling, the electric wind muſt abſolutely be diſtinguiſhed, *which is a real current of air, driven from a point, either annexed, or preſented, to a body ſtrongly electrified.* I. The coolneſs we feel on our hand, ſupplies us with one proof, that this wind really is a current of air, ſince it is the property of air to cool when it is renewed; both, becauſe to an heated air, ſucceeds another which is not ſo, and becauſe to an air rendered damp by the contact of our body, ſucceeds another which, being more dry, is more apt to promote our perſpiration, conſequently to cool us. II. The different kinds of motion which this wind may create, afford another proof of its being a mere current of air; it, for inſtance, drives away the ſmoke of a candle lately extinguiſhed. If, while you are inſulated, and communicate with a body ſtrongly electrified, you rapidly preſent a bodkin to the flame of a candle, the flame will, in the firſt inſtant, be driven by the wind which the bodkin produces; though this will take place, as I ſay, only in the firſt inſtant, becauſe the electricity of the bodkin will ſoon diſſipate through the deferent flame, and the wind will of courſe immediately ceaſe; there will only remain on the bodkin a ſmall electricity, which, inſtead of driving the flame away, will attract it, and through it intirely eſcape. III. The motion of ſmall wheels driven by an electric wind, is ſtill more permanent and ſignificative, becauſe the electrical fire eſcapes in a much leſs quantity through them, than it did through the flame. I make ſuch wheels in a very expeditious manner: I cut a cube of cork two lines broad, and to each ſide of it I adjuſt, with ſome ſpittle, a rectangular piece of wafer, an inch long; I then inſert a needle

through

through the cube, and ſuſpend it by this needle, either with the armature of a load-ſtone, or with a long thin thread; then preſenting the electrified bodkin to the ſides of the wings of the wheel, the latter continues to turn round, and it changes its direction whenever I change the direction of the bodkin.

782. *To the air driven directly, and in the direction of their own axis, by the above bodkin, or by a long needle, another ſucceeds along their ſides, ſo that, beſides the direct wind from the needle, another is to be diſtinguiſhed, which may be called a ſecondary lateral wind.* I. I preſent the wheel to the ſides of the needle, at the diſtance of one, four, or even five inches from its point, and it then turns pretty rapidly; I afterwards preſent it to the other ſide, and the wheel changes its direction.

783. I uſually employ the following experiment, both in order to make the direct wind viſible, and demonſtrate the neceſſity of the lateral wind. I pour oil into the bottom of a china diſh EF (Pl. X. fig. 7.) on the edge of it, I place a lump of ſoft wax, on which lies a rod inclined towards the ſurface of the oil, but which neither touches, nor can attract it, leſt it ſhould thereby become incapable of producing the wind; I then electrify the rod, and preſenting my finger to the ſurface of the oil, at the diſtance of two inches from the point, I ſee the ſurface of the oil depreſſed, and ruffled by the impulſion of the wind that ariſes from the rod. Now, who does not perceive, that as the motion in the inſulating oil is no more than a progreſſive motion of it from the point, ſo the motion of the inſulating air is no more than a like motion, or pulſion of the ſame; it is not true, therefore, that an *effluent* electric matter be the immediate agent that drives away the ſmoke, or puts out a ſmall candle, and that it forms by itſelf the electric wind; an opinion this, however, in which I ſee many a philoſopher perſiſt.

784. I ſpread ſome ſhavings or ſcrapings of a beaver hat, on the ſurface of the oil, and preſenting my finger, as uſual (the diſh is of itſelf inſulating, and, beſides, lies on an inſulated ſtool) and attentively watching for the firſt motion produced by the electricity, I ſee that the rod drives the duſty pellicle of the oil, directly to the diſtance of about two inches, and the effect of it ſoon ex-

tends

tends all round. This motion of the ſurface of a tenacious heavy ſubſtance like oil, which extends to the diſtance of two inches, makes me underſtand eaſily how light moveable air may be driven ſo far as to the diſtance of a whole foot.

785. The beaver duſt which is ſpread on the oil, forms upon it ſmall clots, the different complicated motions of which are pretty agreeable to obſerve; they are firſt driven from the point of the bodkin, to the diſtance of ſeveral lines from it; then they are buried under the ſurface of the oil; when at a certain depth, they bend their courſe back, and, in ſhort, by all their different motions, evidently ſhew the cauſe that produces them.

786. Points, at the ſame time that they drive the air forwards, are themſelves driven back. Monſ. Jallabert was the firſt who obſerved this repercuſſion of points, which the Abbé Nollet afterwards found to be inconſtant. But this very inconſtancy is ſubjected to ſome laws: *in order that a point ſufficiently moveable may retrocede, it muſt be animated by a ſtrong electricity, and a ſtrong electric current muſt actually iſſue out of it, or get into it. But if the electricity of the point be only what we called in former chapters, a preſſing electricity, then the point will be attracted by the body unequally electric that ſtands near it; and in this caſe, the general law of the attraction of bodies, will continue to obtain, for the ſuſpenſion of it in the above experiment, aroſe from the current of the electric wind.* Therefore, if to an animated ſyſtem, you fix the ſharp ſupport B (Pl. IX. fig. 3.) on which the incurvated needle E F C D ſtands equilibrated, you will ſee the latter turn in a direction contrary to that of the electric wind that riſes from it, and exhibit in the dark a circle of light, the rays of which will be the more or leſs extended, according as the needle, or its ſupport, ſhall communicate either with the Chain, or with the Machine, and, conſequently, produce either a bruſh, or a ſtar. If you hold the ſupport in your hand, and preſent one of the points of the incurvated needle either to the Chain or to the Machine, the needle will ſtill continue to turn; but then it will only exhibit an arch of light in that part of the circle it deſcribes in which its points alternately give fire to the Machine, or receive it from the Chain.

CHAP.

CHAP. II.

Of the reafon of the electric wind, and of the retroceffion of points.

787. *THE electric fire runs out of points, or gets into the fame, with fuch velocity, that the direction of it cannot be difcerned either by the naked eye, or with the affiftance of any glafs.* When the incurvated needle (Pl. IX. fig. 3.) is annexed, or prefented, to a fyftem animated by a pretty ftrong electricity, it foon affumes a motion fo rapid as to make it exhibit in the dark, a continued circle of light, fo that it is not poffible to difcern at what part of any given turn the point really begins to lighten, and in what direction it proceeds while it continues fo to do. If we experiment by day-light, and confequently can fee the needle, the impreffion of its electric light on our eyes, is lefs vivid, and lefs permanent; yet the rapidity of the needle is fuch, that inftead of perceiving the direction according to which it turns, we only perceive a continued circle of fhade; this fenfation of fhade, I impute to the weaknefs of the tranfitory impreffion, though, at the fame time, the rapidity of the wheel makes this fhade appear as if continued through the whole circle; it is, therefore, impoffible, under fuch circumftances, both to difcern the direction of the needle, and to receive impreffions exact enough of any particular portion of its courfe, to be thence enabled to perceive whether the perceived light gets into it, or iffues from it; nor could a mifcrofcope be of any fervice to us, the fpace which will be feen through it, will indeed be increafed, but the time in which this fpace will be ran through, will ftill remain the fame.

788. Since I am on this fubject, I afk leave to dwell on it a little longer. In the firft place, we are accuftomed to fee that liquids fpringing from the hole of a veffel, divide and fpread themfelves; it is no wonder, therefore, that the Parifian academicians, perfons fo learned in any other branch of human knowledge, though not fo much converfant with electrical matters, when they faw the effluvia of the electric fire more dilated near

the rubbed globe of glafs, and more united near a key that was prefented to it, thought that the electric fire proceeded from the key to the globe. Introduce perfons not converfant with experiments on light, into a darkened room, in which a ray of light is admitted, howfoever refracted, provided it exhibits the appearance of a cone, and they all will agree in anfwering, that the ray proceeds from the apex of the cone, while it may very well happen that it proceeds from its bafe; now, the fame is likely to have happened with the above Academicians, with refpect to the direction of the fire, in their different experiments. The electric fire depofited by the hand on the glafs globe, cannot unite again on the furface of it, which is infulating, but muft, from the diftant points of this fame furface, run to the key through diftinct convergent lines; and the obfervator, if unacquainted with the whole complement of the facts, will be led to judge, not according to truth, but to his own experience, that the fire fprings from that part to which it yet really runs, merely beeaufe he fees it there to be more denfe and fparkling.

789. The fame is to be faid of the fpurious brufh, into which the ftar will be transformed, in circumftances like thofe related in num. 119. Whoever obferves this brufh for the firft time, is induced by habit to judge it fprings from the point; but whoever afterwards makes the confiderations expreffed in num. 122. foon fees himfelf neceffitated to alter the rules he deduced from cuftom, and to fubftitute others more conformable to the complement of the facts, with which he has now become acquainted.

790. In order to make the fubject more thoroughly underftood by the reader, I fhall now proceed more minutely to examine the different accidents of the motions of the electric fire, when it forms either the *brufh*, or the ftar. *The rapidity with which every particle of electric fire iffues out of a point, or enters into it, is not only confiderable, but the rapidity with which thefe particles fucceed each other, is alfo very great.* Whenever I annex a point to an electrified fyftem, the divergence of the electrifcope leffens proportionally to the greater fharpnefs of fuch point, proportionally to the diftance to which this point reaches beyond the limits of the atmofphere of the fyftem, and proportionally to the vicinity of that fame point to ftrange

ſtrange bodies. I ſometimes annex a wooden frame to the hook Z of the conductor Y, on which I have ſtretched ſome gilt paper, and from the latter I draw ſparks with the knuckles of my fingers. If inſtead of my knuckles, I uſe a ſharp point, which I move with ſufficient velocity towards the gilt paper, to bore a hole through it; then this hole is effected without drawing any ſpark; the reaſon is, that the velocity with which the fire in the paper (which ariſes from the conductor) throws itſelf into the point, is ſuch, that when the point reaches the paper, the fire has already paſſed intirely into it, and there remains none in the paper, or in the conductor, to produce a ſpark. I obſerved, in a former place, that when I diſcharged a coated ſheet of glaſs with a point, though very ſharp, which I rapidly moved towards it, there always remained in the ſheet a pretty ſtrong ſpark; but this only ſhews, that the velocity of the fire is limited, and the above experiment of the gilt paper ſhews ſuch velocity to be extremely great.

791. *The air driven from an electrified point, moves forward to a pretty great diſtance from this point, it being puſhed forwards by other air, which continues to be driven from it; and it is impregnated with the exceſſive fire that flows from that ſame point, if it be electrified by exceſs, or deprived of its own fire, which it gives to the point, if the latter be electrified by deficiency.* If you inſulate yourſelf at the diſtance of about a foot from a flat, or but little convex, part of a ſyſtem animated with electricity, the atmoſphere actuated around it will, as we have formerly ſeen, introduce a contrary electricity into that part of your body which is immerſed in it. But if you inſulate yourſelf, facing a point that ſprings from ſuch ſyſtem, the wind that blows from that point will introduce into your body the ſame kind of electricity as that in the ſyſtem, and introduce it at the diſtance of five, or even ſix feet; which introduction will be the quicker, as the diſtance will be leſs, and the point ſharper, and extended farther beyond the limits of the atmoſphere of the ſyſtem.

792. If you take a rolled piece of metallic foil, and keep it inſulated by a long, very thin, and dry ſilk thread, and preſent it to an electrified flat body, it will hang according to its natural direc-

tion, becaufe bodies attract each other only when contrarily electrified, and repel each other only when fimilarly electrified; if you touch the rolled leaf, it will fly to the fyftem, as it is now become contrarily electric. But if you now prefent the fame to a point, even three feet diftant, you will fee it repelled by it; touch it, and it will hardly have begun to move towards the point, when it will fly from it.

793. Hence we fee, that one of the greateft difficulties that remained to be explained concerning electricity is removed. Indeed it was a thing very difficult to conceive how it came to pafs, that while a fpark, which appears fo active, can only pafs through air, to the diftance of an inch, the thin effluvia from an electric brufh (which may be brought to fuch tenuity as, at the diftance of an inch, to become wholly invifible) fly by this means to the diftance of feveral feet. The fact is, that they do not run to fuch a diftance, driving the air all along, by dint of their own continued motion, but are tranfported fo far, by the general motion which the electricity of the point communicates to the ambient air, and continues to maintain in it.

794. In the fame manner, if the point to which I prefent my hand, at the diftance of three feet, be electrified by deficiency, the fire does not run directly from my hand to fupply this deficiency, through this interval of three feet; but it is the natural fire of the air, which, from the fides of the point, continually flows to the apex of it, that diffufes itfelf into it; and it is this air, thus deprived of its natural fire, which, being fucceffively driven towards my hand, as it were carries the deficiency of the point to it, fo that my body, if I ftand infulated, becomes electric by deficiency, becaufe continually new particles of fire diffufe themfelves from it into the air which is brought by the above wind, deprived of its own natural fire. Hence we may alfo deduce a folution of another difficulty ftill more weighty, concerning the electric wind itfelf, which, as we fee, equally blows *from* points electrified by excefs, and by deficiency.

795. *The natural fire which, from the air contiguous to a point diffufes itfelf into it, muft needs drive forwards the air which lies around*

around the point, and repercufs it in the fame manner as it happened when the point was electrified by excefs, and the electric fire rufhed from it into the contiguous air. Univerfally, whenever the electric fire moves through fubftances in any degree refifting, it drives their particles on all fides, but efpecially in that direction in which it meets with the leaft refiftance. This truth neceffarily follows from the natural expanfive force of the electric fire, and is in all cafes warranted by experiment.

796. Now, if the electric fire, when paffing condenfated to a great degree, through bodies or mediums, has the propriety of driving their particles into contrary directions, it certainly muft equally exert this propriety, both when it paffes from the air into the point, or from the point into the air, and drive them both into oppofite directions, after the fame manner as in a former experiment, the pieces of a little glafs tube which contained a certain quantity of water, were, as well as the water, driven into oppofite directions.

797. The reader may alfo here recollect the experiment before related, of the quire of paper through which a difcharge was fent; whatever the direction of the fpark was, either upwards or downwards; the brim of the hole was turned upwards in all the leaves except the laft, in which the bur was turned downwards. This experiment fhews that the center of the action of the fire, whether it iffues from the point, or gets into it, lies at a certain fhort diftance from the top of this point; and the fire, from fuch center, drives the particles of the refifting medium on all fides.

It, therefore, appears, that the celebrated queftion, why points drive the air forward, whatever the direction of the fire may be, is reduced to the univerfal fact, viz. that the electric fire drives the refifting particles of the medium through which it moves into contrary fides; we, in fact, fee, that the electric fire, in all cafes, drives both the air and the point; but for the more complete explanation of this fubject, there remain a few experiments more to be related; which will be done in the following chapter.

CHAP. III.

The reaſon of the bruſh, and of the ſtar.

799. *AN homologous atmoſphere, if it ſufficiently ſurrounds the point, ſuppreſſes the wind, the ſtar, or the bruſh, and hinders the electricity, whether by exceſs, or by deficiency, from diſſipating itſelf through this point.* Preſent a glaſs tube ſtrongly electrified to a point annexed to the Chain, ſo that the atmoſphere of it amply involves the point; or to a point adapted to the Machine, preſent a ſtick of ſealing-wax ſtrongly electrified; and the wind will immediately ceaſe, the ſtar or bruſh diſappear, and the electroſcopes ſoon acquire their greateſt divergence.

800. Nor is it neceſſary that ſuch bodies as are preſented to points, ſhould be actually electrified, it is enough that they be inſulating, and ſufficiently extenſive. I bring á glaſs ſpoon very hollow, near to a point, ſo that the apex of the point be incloſed within the brim of the ſpoon; at the ſame inſtant the light diſappears, and the electricity in the ſyſtem to which the point is annexed, increaſes; the reaſon of this is, that the ſpoon ſtops the air either loaded with the fire of the point, or deprived of its own; which is ſufficient to ſuppreſs any vivid electricity from the point.

801. Hence it alſo happens, that a point turned backwards, and directed towards the ſyſtem itſelf to which it is annexed, ceaſes to give, or to draw, any fire.

802. *An homologous electricity obliquely directed to a point turns the wind, the bruſh, or even the ſtar, to a contrary ſide.* I adapt to the conductor Y (Pl. X. fig. 8.) a rod which I faſten to it in an oblique ſituation; and I obſerve, that in proportion as I make the angle compriſed between it and the conductor more acute, the bruſh and the wind deviate more from the axis of the point towards the oppoſite ſide.

803. Thence I have underſtood an irregularity, which I obſerved with regard to the incurvated needle E F C D (Pl. IX. fig.

fig. 3.) I placed it on an electrified fyftem, and with fome fealing-wax I fixed its extremity D; then prefenting a little wheel laterally to the prolongated axis of the point E, I was amazed to fee that the wheel either did not move, or moved in a direction quite contrary to what I expected. The fact is, that the atmofphere of the body FC of the needle, made either the wind, the brufh, or the ftar, deviate confiderably; fo that in order to make the little wheel turn, it ought to be prefented, not to the prolongated axis of the point of the needle, but to the wind itfelf, which, owing to the particular fhape of the whole needle, was fomewhat obliquely directed.

804. Thence we may alfo underftand why, when we infert a point through a fmall tube of glafs, the orifice of which extends fomewhat beyond the apex of the point, and prefent our finger to it, the brufh, or the ftar, degenerate to a number of brifk fmall fparks. The electricity from the point foon fills the tube, and there arifes all round the point, and clofe to it, an homologous electricity which foon fuppreffes the electricity of it.

805. An homologous atmofphere which only extends towards the fyftem to which a point is annexed, increafes the wind, the brufh, or the ftar. Adapt a point to the conductor A 3 B (Pl. VIII. fig. 8.) let three men rub a glafs-tube each, and jointly prefent it, the one to the back part of the conductor, and the two others, along each fide of it; if the operation be made in the dark, you fhall fee a brufh fpring from the point, though there might be before no electricity in the conductor; if the latter be electrified in any degree, you fhall fee the brufh fparkle with the more brifknefs.

806. *Contrary* atmofpheres *fimilarly* applied, produce contrary effects; applied after a contrary manner, they produce fimilar effects. I. A contrary atmofphere directly applied to a point, quickens the wind from it, as well as its brufh or ftar, and increafes the diffipation of its electricity. II. Being obliquely prefented to the point, it draws to itfelf the wind from it, as well as the ftar, or brufh; and in proportion to the intenfity of its oblique action, it quickens the diffipation of the electricity. III. Being prefented to the furface itfelf of the fyftem to which the point

point is annexed, it retards and leſſens the diſſipation of its electricity.

807. Theſe effects of contrary atmoſpheres I content myſelf with mentioning, becauſe they are manifeſt; beſides, the experiment formerly related concerning homologous atmoſpheres, would alone ſuffice to lead us to the diſcovery of the following important truth, viz. *that the diſſipation of the electricity of bodies through points, is owing to the electric atmoſpheres of thoſe bodies, in conſequence of the reaction which they exert againſt them; which reaction is much greater along the flat parts of ſuch bodies, than around their ſharp parts, or the points annexed to them.*

808. I ſhall examine, in the firſt place, the caſe of a point annexed to a ſyſtem electrified by exceſs, which throws a bruſh; though I have in a former place laid down the principles on which the above explanation is grounded, yet, in order to procure the reader a greater evidence on the ſubject, I ſhall here lay them down again. Firſt, I obſerve, that the exceſſive fire which flows out of a point A (Pl. IX. fig. 1.) of a right line BC, or of a plain ſurface, does not actuate an electricity by exceſs in the ambient air, by diffuſing itſelf into it, through a right line AD, or by ſpreading itſelf through it to any diſtance, along any lines AE, AD, AF, but that it ſtops in A on the ſurface of the body, and that from thence, without leaving the place A, it introduces a particular kind of tenſion into the fire proper to the air around this body; theſe facts, I think, I have ſufficiently demonſtrated in the third ſection of this book.

809. I obſerve, in the ſecond place, that this tenſion I mention gradually leſſens in a certain proportion to the different diſtances of the body, ſo that it ceaſes ſomewhere, for inſtance, in EF, in conſequence of the *vis inertiæ* which is inherent in the electric fire, as well as in other ſubſtances.

810. In the third place, I obſerve, that ſuch tenſion will take place not only in the fire proper to ſuch portion of air AD as is directly oppoſed to the point A, and in the lateral ſtrata of it AE, AF, but will communicate itſelf backwards through this air, after the ſame manner as any kind of motion is propagated through fluids; on which the reader is deſired again to peruſe the paragraphs 452—455 of this work.

811. This

811. This important obſervation ſupplies the ſolution of the intricated knot or difficulty we are now handling. I. The fire proper to the ambient air, being brought to a particular degree of tenſion by the electricity which is actuated in A, exerts a re-action againſt that ſame electricity. II. And that electricity which is actuated in A, muſt needs, by the means of the tenſion which it introduces into the fire proper to the ambient air, and which it propagates to all ſides, alſo exert a reaction againſt the fire which ſeeks to iſſue from the adjoining points G, I, K, H, B, C, &c.

812. Now, the concluſion I draw from the above principle, is the following. If the ſurface, however varied, of a ſyſtem be conceived to be divided into a number of equal exceeding ſmall particles, and the adjacent atmoſpheres be conceived to be divided in the ſame manner, the exceſs which is exerted from each of theſe particles, experiences a reaction proportioned to the number of theſe different atmoſpheres which co-operate in repercuſſing it, to the leſs obliquity with which they act, and to the leſs diſtance from which they do ſo. Therefore, the whole reaction experienced by the exceſs that ſeeks to ruſh out of narrow hollow places, will be relatively infinite (the reader is deſired here to recollect the former experiments on the electric *well*) becauſe, though the number of the atmoſpheres may grow leſs as the cavity grows narrower, yet they then act more directly againſt each other, and from leſs diſtances. III. This reaction will, therefore, leſſen as the cavity will grow wider. IV. It will leſſen ſtill more when the hollow place will become convex. V. And its value will be infinitely ſmall when the convexity will become infinite, and, beſides, reach beyond the limits of any atmoſpheres belonging to other parts of the ſyſtem; that is to ſay, when the above convex part will be turned into a very long ſharp point.

813. In the fig. 1. pl. IX. let A B C be the line that would be repreſented on the ſurface of a long ſharp rod by a ſection made parallel to its axis. I. Let ſuch line be conceived as divided into as many particles B *b*, equal to the tip C of the point of the rod, as it may contain. II. From all theſe points let the perpendicular equal *electric lines* be excited, B F, *b f*, &c. C G, D A, E C, and then

then the half part of the angle E C Q will exprefs the fuperior obliquity (relatively to the obliquity with which the lateral atmofpheres act againft each other) with which the atmofpheres around the point act againft the excefs that feeks to rufh from it; which, as the reader may eafily perceive, is exceedingly fmall.

814. If we pafs to examine the number of all thofe atmofpheres, actuated around the fuperficies of a point, and which obliquely direct their reaction towards the excefs which endeavours to iffue from the top C of it, we fhall find, firft, that the number of thefe fuperficial atmofpheres will gradually leffen near this top, in proportion as the latter will taper to a fharper point, and the geometrical proportion followed by fuch decreafe might, if proper *data* were affigned, be eafily inveftigated and difcovered.

815. Whence, if we finally join the confideration of the fmall oppofition experienced by the excefs that endeavours to iffue out of the top C, in confequence of the great obliquity of the lateral atmofpheres, with the confideration of the fmall number of the fame, we fhall eafily fee how it comes to pafs, that the great reaction exercifed by electric atmofpheres againft the excefs which endeavours to iffue out of the plain furfaces of a body, muft make a great part of the redundant electricity in that body rufh from a point annexed to it, where this reaction or oppofition from atmofpheres is much lefs, fo that a brufh takes place on fuch point, thence alfo arifes an electric wind, and a repulfion of the point, which always is a confequence of this wind.

816. This efficient caufe of a brufh is the fame as that which produces fparks (and as we fhall fee hereafter, the fame with that of the other electric figns) inafmuch as both are caufed by the reaction of the electric atmofpheres of the body on which they take place, againft the fire in that body, which makes fparks leap from plain, or but little convex, parts of the fame, or a brufh fpring from a point annexed to it; however thefe fparks and brufh differ from each other in this important refpect; the former only rufhes out, when a ftrange body, for inftance a finger, is approached, and thus leffens the reaction of the atmofphere againft the place whence the fpark we fuppofe is thrown (which it effects by fhortening the column of air contained between it and the body) whereas the

fhape

ſhape alone of the point from which the bruſhes ruſhes out, is able to leſſen the re-action around it, ſo that the fire continually ſent to it from the electrified body, may ſucceſſively run againſt the column of ambient air, however extended, and blow it away while mixing with it.

817. The ſame cauſe may alſo be aſſigned to all the accidents of the bruſh, and firſt to the direction and ſhape of the ſtream of fire that forms it; the lateral atmoſpheres of the point A C B (Pl. IX. fig. 1.) exerciſe an oblique *re-action* along the ſpaces C E H, C G I, by virtue of which, I. The bruſh is confined to the ſpace H C I. II. The electric wind blows chiefly in that ſpace H C I. III. and the point is alſo repercuſſed in that ſame direction, which is owing to the air being driven towards oppoſite parts, in the direction of that line, along which the ſtream of fire is determined to flow, by the re-action of the lateral atmoſpheres. In fact, as a ſtrange homologous atmoſphere preſented ſidewiſe to a bruſh, ſuffices to make it deviate towards the oppoſite ſide, ſo the oblique action of lateral equal oppoſite atmoſpheres will have the effect of retaining the bruſh on the prolongated axis of the point, and ſettling it there.

818. The truth of the cauſe to which we here impute the narrowing of a bruſh which ſprings from a ſharp point, is confirmed by the divergence a bruſh aſſumes, when ſpringing from a blunted point, becauſe on ſuch points ſeveral diſtinct atmoſpheres may take place, which thus reciprocally confine the bruſhes or fire which they endeavour to raiſe within the body of the point. Inſtead of a point, make uſe of a braſs rod, a line and an half thick, the top of which is quite flat, and you ſhall ſee the bruſhes ſpring only from the circumference of this top, and ſpring very divergent and widely ſpread, being continually repelled by the atmoſpheres that take place on the flat ſurface of the ſame.

819. Now, in comparing the narrow bruſh which ſprings from a very ſharp point, with the ſeveral ſmall ones that ſpring from a blunted point, we might, if we were to truſt our ſenſes, be led into an error concerning the real quantities of fire that are thrown out; the numerous bruſhes that ſpring from the circumference of a blunted point, ſeem to indicate a diſſipation of fire much more

copious than can well be made through the ſingle narrow bruſh that ſprings from a ſharp point; but experience teaches us that the caſe is quite otherwiſe. I inſulate myſelf, and communicating with the Chain, I ſtrike a ſheet of gilt paper, ſtretched on a wooden frame, firſt with a needle only, then with two, with three, with four, and with ſix, taking care that the points of all theſe needles be in the ſame plan; and in proportion as the needles I uſe are more numerous, and form a thicker bunch together, I give the gilt paper a larger ſpark. This aggregation, or bunch, of points, may very well be conſidered as one blunted point; and in proportion as the width of its top increaſes, the ſpark alſo increaſes which I give through it to the gilt paper; now, with a ſharp point, I never can give any; a certain indication this, that through this ſingle point all the fire in me eſcapes in an inſtant, whereas it does only diſſipate ſomewhat ſlowly through the aggregation of points.

820. In attentively comparing the different circumſtances which accompany the narrow bruſh that ſprings from a ſharp point, with thoſe of the ampler or compounded bruſh that ſprings from a blunted point, it will not be difficult to remove the difficulties that may ariſe from their different appearances. In the former bruſh you ſee a continued united ſtream of fire; in the latter, you only perceive a kind of tremor, or quivering motion, and the flaſhes of the fire that form it are alternately ſhortened and lengthened. If you conſult your hearing, you will hear the ſharp point emitting a continued hiſſing, the blunted one only a rattling, formed (as we obſerved in the firſt ſection) of ſucceſſive diſtinct ſmall cracks. If you inſulate yourſelf, and then attempt to draw fire from a blunt point, you ſhall find that it is not able to ſend fire to you, but from a leſs diſtance than a ſharp point.

821. All this is intirely conformable to theory, and to what has been above ſaid with regard to the different degrees of reaction exerciſed by atmoſpheres; which reaction depends on the ſhape of the bodies around which theſe atmoſpheres are actuated.

822. But

822. But when the electricity is very intenſe, another ſeeming contradiction takes place; in ſuch caſe, bruſhes ariſing from a blunted point ſpread very wide, whereas thoſe from ſharp points are confined to a ſtill narrower ſpace, they even loſe their length, and are at laſt changed into imperfect ſhort bruſhes, or ſtars. But it lies in my power to reſtore to ſuch bruſhes their natural form; I only need put a piece of paper, or a bit of ſoft wax between the ring Z of the conductor Y (Pl. I. fig. 1.) and the hook by which the point hangs to it; the deceiving ſtar then immediately reſumes the appearance of a real bruſh; or I only need leave rubbing the glaſs, and, at the ſame time, that the electricity grows languid, the bruſh lengthens itſelf.

823. This transformation of a bruſh into a ſtar, often happens even in the blunt points annexed to the wires with which I explore the electrical ſtate of the atmoſphere, when flaſhes of lightning take place; this proceeds from the vehement electricity which then ariſes in each of the many points that may be ſuppoſed to form the blunt point, and is an effect which the milder electricity of a conductor cannot produce upon more than one.

824. As the ſhortened bruſh hiſſes more ſtrongly than a long one, ſo it exhibits in its ſhort tract a more intenſe light; it drives the air to a greater diſtance, after it has impregnated it with its own fire. It is evident, that the ſhortening of the rays ariſes from both the greater denſity and greater velocity with which the fire ruſhes from the point; that is to ſay, as an aggregation of any corpuſcles whatever, penetrates the leſs deeply into water in proportion as this aggregation is more compact, and ſtrikes the water with more force, ſo, the more the ſubſtance of the electric fire is united together and the more rapidly it moves againſt the ambient air, the leſs deeply it divides it, and penetrates into it; though it drives it forwards to a greater diſtance, and more quickly eſcapes away in company with it, by virtue of the motion it has communicated to it: now, when the electric fire moves by virtue of this latter kind of motion alone, it even makes no impreſſion on our eyes, and eſcapes in an inviſible manner.

825. The *unity* to which theſe explanations which riſe from the fact itſelf, bring the various accidents of the bruſh, I conſider

as a new proof of the truth of the theory above laid down with regard to that electric ſign. I ſhall now conclude on this ſubject, with relating an obſervation of Mr. Kinnerſley (Franklin, p. 337.) which appeared at firſt a bare unconnected fact, and which, the theory above being underſtood and admitted, becomes an obſervation pregnant with luminous conſequences. Let an inſulated perſon hold a point, the apex of which is turned outwards, and remains near his body, and it will only exhibit a very ſcanty languid degree of light; but as ſoon as the perſon ſtretches his arm, the light greatly increaſes. Mr. Kinnerſley imputed this to the more copious electricity which was diffuſed in the air near the body of the perſon; but now, when it is demonſtrated, that the *actuating* electricity remains confined to the ſurface of bodies, this explanation cannot hold; it is, therefore, clear, that the fact muſt be attributed to the *re-action* of the atmoſpheres, as explained above.

826. The ſame obſervation, which is common both to points electrified by exceſs, and to thoſe electrified by deficiency, leads us to apply the ſame theory to the latter that has been hitherto advanced with regard to the former. In points annexed to a ſyſtem whence the electric fire is drawn, the direction of the actuating force is inverted, and that of the actuated fire muſt of courſe be alſo inverted. I. Correſpondently to that portion of electric fire which is drawn from a part of a body, either plain, or but little convex, the natural tenſion in the air contiguous to this part muſt be relaxed. II. This relaxation is propagated to a certain diſtance, with a ſuitable diminution, and is alſo turned laterally, and to all ſides obliquely, againſt the ambient air, contiguous to the ſame body; it, therefore, oppoſes a reſiſtance to the force that endeavours to draw the fire from that body. III. Now, if we apply to this caſe what has been above ſaid with regard to the bruſh, it is plain, that the reciprocal oppoſition we mention, will be greateſt and relatively infinite in narrow cavities; that according as thoſe cavities ſhall increaſe, the oppoſition will leſſen; when the cavities become plain ſurfaces, it will leſſen ſtill more; when they become convex, it will be ſtill leſs; and when infinitely convex, that is, when they become changed into very ſharp points that reach

reach quite out of the atmofpheres of the body to which they are annexed fuch oppfition will become infinitely fmall; therefore the force that will draw the natural fire at the apex, will become relatively infinite; that is to fay, this force will be able to draw the natural fire from the contiguous air, and thus a ftar will take place.

827. The characteriftic mark of this electric fign will, in proper circumftances, be the fhortnefs of its light; but before I treat this fubject, I muft anticipate a difficulty which will naturally be propofed againft the above explanation of the brufh and the ftar. Hitherto I have only fpoken of points annexed to electrified fyftems; but the fact is, that a point prefented from the ground to the Chain, will exhibit a ftar juft the fame as if it were annexed to the Machine; and a point prefented to the Machine will throw a brufh in the fame manner as one annexed to the Chain; now neither the fire in the air that furrounds the man who prefents a point to the Machine, is brought to any particular tenfion, nor is that of the man who prefents a point to the Chain brought to any degree of relaxation; it, therefore, feems, that in both thefe cafes the efficient caufe fails, which we have above affigned to thefe two electric figns.

828. But this difficulty cannot be propofed by perfons who have attentively confidered the theory of electricity. The natural tenfion of the fire in the air that furrounds my body, may very well be either exceffive, or deficient, *relatively* to the tenfion of the fire around the apex of the point, though there is in myfelf no abfolute electricity. When I prefent a point to the Chain, this point becomes immerfed in an air, the fire of which poffeffes a vehement tenfion, and is ftrongly vibrated towards the point; therefore with regard to fuch tenfion, the natural tenfion of the fire around my body, is a relative relaxation. In the fame manner, when I prefent the point to the Machine, it finds itfelf involved in an air the fire of which is much relaxed, efpecially near the Machine, and is ftrongly folicited towards it, as the latter is in a wanting ftate, &c.

829. Thefe principles being laid down, it becomes pretty eafy to determine the circumftances in which the ftar keeps its characteriftic appearance, viz. that of exhibiting fhort and fcarcely difcernible

rays.

rays. I. On a very ſharp point, whether it be annexed to a ſyſtem electrified by deficiency, or preſented to one electrified by exceſs, there always appears a real ſtar, whatever may be the diſtance (provided it be not too great), and the ſhape of the particular part of the ſyſtem towards which it is directed. II. The ſame is to be obſerved with regard to blunt points. III. Univerſally, whenever a point draws only ſuch fire as lies in the contiguous air, this fire exhibits the appearance of a ſtar; it can preſent the appearance of a bruſh, only when it leaps to the point from the electrified body itſelf, and unites again near it, dividing the air by dint of its own motion; now, this can never be the caſe with a ſharp point, becauſe, while it is bringing to the electrified body, it ſo readily draws the fire in it, that with whatever rapidity it may be preſented, the whole exceſs in the body is abſorbed by it, before it comes to a diſtance ſufficiently ſhort to produce a bruſh: it is only a blunt point, or one which is imperfectly deferent, that can come near the ſyſtem, and yet allow a pretty conſiderable portion of its exceſs to continue; which exceſs leaps to it, by virtue of its own motion, as ſoon as the diſtance is become ſhort enough, and then exhibits the appearance of a ſpurious bruſh.

830. The above explanations concerning blunt, or imperfectly deferent points, may be applied to the different experiments in which the Pariſian Academicians thought they ſaw the electric fire run from points to rubbed glaſs globes, in the ſame manner as it would to a conductor negatively electrified (Nollet, *Lettres ſur l'Electricité, ſeconde partie, pag.* 253.) The top of a finger, the ring of a key, the edge of a ſilver crown, which they preſented to a globe, where more or leſs blunt bodies; a piece of green wood, the extremity of a wetted rope, a piece of paſteboard, &c. were bodies imperfectly deferent; and the head of the metallic ſquare rod, eight lines thick, which they uſed, had in its angles and ſides a kind of aggregation of points more or leſs blunt. All theſe bodies, beſides, were preſented to the globe at a diſtance probably very ſmall, ſince it is ſaid of the ſquare rod, that it was not more than half an inch diſtant from it (pag. 254.) it is not, therefore, to be wondered, that if the rays of the electric fire, which the hand

hand left on the globe, leaping through the inconſiderable ſtratum of air before them, and uniting together on the above mentioned bodies, exhibited ſpurious appearances of bruſhes; which induced the Academicians to judge that the fire, inſtead of proceeding from the glaſs globe to the bodies, diffuſed itſelf from the bodies into the globe.

831. But amongſt the experiments which the Abbé Nollet made on this ſubject, in the preſence of the Academicians, there is one which alone ſuffices to reſolve the difficulty, and ſhow the real direction of the fire when it forms theſe ſpurious bruſhes. We took, ſays the Abbé, pag. 254, num. 16, a ſquare iron rod, ſix feet long, and eight lines thick, and employed it as a conductor. The bruſh having ſpontaneouſly ſpread itſelf at the remoteſt extremity of the rod, always grew larger and more luminous when ſomebody preſented his hand, his face, or any other broad part of his body to it (num. 18.) But when (num. 19.) the ſame perſon preſented the top of his finger, or a piece of metal of a like ſhape, at two or three inches diſtance from the conductor, then the bruſh which iſſued from one of the angles, directed itſelf towards the body that was thus preſented, and its rays, naturally divergent, bent themſelves towards it, as if ſeeking to embrace it. If the finger, or piece of metal, was preſented ſtill nearer, the rays at laſt contracted themſelves ſo, as to form a ſingle ſtream of a denſer and more inflamed matter.

832. In this experiment, the Academicians ſaw with their own eyes, and confeſſed the unity of the direction of the fire when it gathered on the finger, and there exhibited the appearance of a ſpurious bruſh; now the ſame appearance muſt alſo have taken place when the finger was preſented from the ground, either to a rubbed glaſs globe, or to a broad flat part of an electrified body, or conductor. The only difference was, that in the laſt caſes, the rays of electric fire ſprung divided from the larger ſurfaces of the bodies we mention; whereas, when the finger was preſented to the angle of the conductor, the rays only became again united together, and then the ſpurious bruſh took place, into which the true one became transformed, by its divergent rays being thus made again to gather and unite together.

833. The

833. The obfervation added by the Abbé, in the following paragraph, that the finger, or piece of metal, when thus held oppofite to a brufh, appeared covered with luminous points, whence rays proceeded almoft fimilar to thofe of the brufh, is of no moment; thofe rays were nothing more than the rays themfelves of the brufh, which, by bending themfelves towards the finger, and converging to it, may, nay muft, have covered it with luminous points.

834. I fhall only add, as a conclufion of this fubject, that, of the experiments made by the Academicians, which is contained in numb. 31. Whenever fparks were excited between a conductor and a body not electrified (thefe are the words of the Abbé), if both were of a confiderable bulk, and every circumftance favourable to electricity, we obferved that the ftream of fire came as well from the one as from the other. With regard to this affertion, I fhould think it an affront to the vifual faculty of the reader, were I to offer a fingle word.

835. I have rather to conclude this chapter with affigning the caufe of the fhort rays of a real ftar, and the long rays of a real brufh; this caufe manifeftly appears from the various circumftances in which, as I have before related, thefe electric figns retain their real appearance, or affume fallacious ones. The ftar appears, whenever a point draws the electric fire from the neighbouring air; now, *the force with which the infulating particles of air retain their own fire (which never quit hold of it, but like other infulating fubftances, from a fmall depth) is the caufe of the fhortnefs of the rays of fuch ftar.* The brufh appears whenever exceffive fire is thrown into the neighbouring air; *now, the force with which the electric fire runs forwards through this moveable contiguous air, is not by affixing itfelf to it, but by dividing it; hence the extenfion of the rays that form the true brufh.*

SECTION

SECTION VI.

On electric motions.

836. THE electric motions by which bodies approach each other, or recede from each other, in air which is free from electrified vapours, may be reduced to the two following laws. I. Two bodies ſimilarly electrified, recede from each other in proportion to the ſums of their electricities, directly, and of their maſſes, inverſely. II. Two bodies contrarily electrified, move towards each other, in proportion to the ſums of their contrary electricities, directly, and of their maſſes, inverſely. In air impregnated with electrified vapours, the two aforeſaid laws are liable to an exception, but which yet is of a pretty ſimple nature. The laws concerning more intricated motions, are only compounded of the laws above. However, as this ſubject is very extenſive, and ſince with the explanation of the above laws, we are to combine the extenſive law of the *vindicating* electricity, which will be laid down hereafter, I propoſe to divide the preſent ſection into two parts.

Firſt part of the Sixth Section.——On the electric motions of deferent bodies.

CHAP. I.

On the law, and meaſure of the retroceſſion of deferent bodies, in open air.

837. I. I Unite together the angles of a ſilver leaf A, and keep them thus joined with a long thin ſilk-thread; the other extremity of which I faſten to a hook Z (Pl. I. fig. 1.) of the conductor Y, and I take care that the leaf be placed at a great diſtance from any ſtrange body. I then graſp the belly of a

bottle, the hook of which is terminated into a ball; and with the other hand I hold, by a fimilar filk thread, another leaf B, exactly like the former A.

838. With the above ball, I touch the leaf A, and thus electrify it; then, ftretching my hand, I gradually carry this leaf A near the leaf B. So long as the diftance between both remains greater than an inch and an half, the two leaves hang parallel to each other; whence I perceive, that, *between two bodies, the one of which is electrified, the other not, no electric motion takes place.*

839. Another perfon prefents his finger to the leaf B, and withdraws it as quickly as he can; then the two leaves run and meet each other. As foon as a contact has thus taken place between them, they mutually recede from each other, and if the upper extremities of the two filk threads be joined together, the two leaves exhibit a confiderable divergence; that is to fay, the leaf A with its atmofphere endeavours to drive the natural fire from the next furface of the other leaf B, into the oppofite furface of the fame; whence refults on this oppofite furface an atmofphere by excefs, which expells the natural fire from the finger that has been prefented to it; the leaf B therefore runs to this finger which now is in a wanting ftate, and gives fome of its own fire to it; as the two leaves are thereby brought to a different electrical ftate from each other, they move to each other, and by this means *becoming again fimilarly electrified, again diverge.*

840. IV. I remove the leaf B from the leaf A, and touch either of them, with the hook (or ball) of the charged bottle; when both the leaves, if brought clofe to each other, diverge ftill more than formerly: the reafon is, that, in the former inftance, the two filver leaves diverged in confequence of the electricity of the ball, or hook, communicated to A, and thence communicated to B; whereas they now diverge by virtue of both the remnant of the former electricity in the one, and of the whole electricity juft now diffufed into the other.

841. V. I now alfo touch the other leaf with the ball; and when both are brought near each other, their divergence increafes ftill more: whence I infer, that *the mutual receffion of two bodies, fimilarly electrified, is proportioned to the fum of their refpective fimilar electricities.*

842. Thefe

842. Thefe experiments muft be made in very favourable weather, becaufe otherwife the atmofphere of A may drive the natural fire of B up the filk thread by which it hangs; and then attract it as foon as it is brought near it: it muft be obferved farther, that in order to caufe one of the leaves to run to the other, and thus draw its electricity, it muft be firft touched by another perfon, that it may through him fpeedily fend away its own natural fire; otherwife it would not move towards the other body but with great flownefs.

843. With regard to the proportion that obtains between the mutual receffions of deferent bodies, and the fums of their fimilar electricities, I fhall, at the end of this chapter, relate a few more precife experiments. I only propofed here to confider the compounded receffions of two or more bodies fimilarly and equally electrified; that is to fay, the mutual receffion which is produced in thefe bodies, not only by their own atmofpheres, but is in each of them modified by the action of other neighbouring homologous atmofpheres, and alfo by the particular actions of gravity.

844. I. Even the mutual receffion of two equal flaxen threads, fixed to the fame hook Z of the conductor Y (Pl. I. fig. 1.) is a compound motion; becaufe they are repelled, not only by their own reciprocal atmofpheres, but alfo by that of the conductor: therefore, if the conductor be horizontally fituated, they recede from each other in a vertical plane, perpendicular to the axis of the conductor: the reafon of this is, that it is in this plane that the atmofphere of the conductor, which follows the fhape of it, is leaft extended, and its repelling power is leaft: the threads are therefore carried and fupported in this plane by the excefs of the repulfion which arifes from oppofite parts of the conductor.

845. Nor do the threads diverge as much as fhould naturally be expected from the action of their atmofpheres; the amplenefs of the conductor, to which correfponds the amplenefs of its atmofphere, laterally repells thefe threads, and thus leffens the angle of their divergence, which increafes in proportion as this atmofphere is lefs extended.

846. From thefe fame principles it follows, that if two threads are annexed pretty near the extremity of the conductor, they will

deviate towards this extremity, fince the action of the conductor's atmofphere will, in this cafe, be more extended and efficacious on the one fide of the threads, than on the other.

847. Let the two threads hang from a point A (Pl. IX. fig. 12) in the middle of the lower furface of the conductor Y, which is inclined to the horizon. Let the line N A be drawn perpendicular to the lower furface of the conductor, and the line A V be drawn perpendicular to the horizon; then the two threads A B, A C, will diverge in a plane B A C, which will be contained between the two lines A N, A V: the gravity of the threads endeavours to make them move in a plane belonging to A V; the action of the atmofphere of the conductor endeavours to make them diverge in a plane belonging to the perpendicular A N; they therefore will diverge in a plane that will be in the middle between both.

848. In the figures 13, 14, 15, 16, the right line I M expreffes the lower fide of a cylindric conductor four or five inches wide, and horizontally fituated; the point A expreffes a middle point on this lower fide, to which three, four, &c. threads are faftened. Let us fuppofe that only three fuch threads are fufpended to A (fig. 13.) the points B, C, D, may exprefs their lower extremities in the fame manner as if they were feen projected on an horizontal plane placed under them: the two threads B D will diverge to oppofite fides and deviate from the vertical line, in confequence of their being repelled by the thread C; which in its turn will be repelled by the two others; though this repulfion or deviation of the threads, will be much leffened by the action of the atmofphere of the conductor.

849. If to the point A (fig 14) four threads are annexed, the points B, C, D, E, will exprefs the places of their divergences; two of thefe threads will fimilarly diverge on each fide, and all will equally deviate from the point A; but the excefs of the action that refults from the length of the atmofphere of the conductor, will caufe the diftance of B from C, and of D from E, to be lefs than the diftances C D, B E.

850. If to the point A five threads be adapted, (fig. 15) four will diverge laterally from A, one will diverge in a vertical plane made on I M; but two of the lateral ones C and D will diverge

lefs

lefs than the two others B F, which will be fupported and kept out by the atmofphere of C and F, and of D, and F.

851. To the point A (fig. 16) let fix threads be fufpended; they will diverge laterally, three on each fide, and all fix will form an exagon; only the fides B C, B D, G F, F E will be fhorter than the fides D E, B G, by virtue of the increafe of repulfion that will arife from the length of the conductor's atmofphere.

852. But the moft ftriking experiment, is that reprefented in fig. 10 and 11 of the fame Pl. IX. A, B, C, D, E, F, &c. are a feries of threads, that hang parallel from the lower furface of a conductor, and are diftant from each other about two inches. When the electricity is fent into the conductor, the threads move from each other, alternately parting, the one on the left, the other on the right fide of the conductor, as is reprefented in B, D, E, H, L, N, P, R, &c. (fig. 10) and form as it were, two rows, D, H, N, R, &c. B, E, L, P, &c. Befides this diftribution, and mutual receffion of the threads on both fides, we may farther obferve, that the threads at the extremities, B, D, &c. P, R, &c. from the middle threads deviate outwards, becaufe there are no other adjoining threads that may repell them, in the fame manner as they are repelled by thefe middle threads. Certainly, if we were to inveftigate the different degrees of action which atmofpheres of different fhapes exercife againft each other, we would find that thofe compound actions accurately follow the principles laid down in the theorems of mechanicians concerning compound preffions.

853. I fhall add here a very fimple trial, which I have made to meafure the proportion between the divergence of two threads, and the quantity of the electricity that acts upon them. I. In the middle of the hall allotted for experiments in the Royal Univerfity, I fufpended with long filk ftrings, X Y, X Y, (Pl. V. fig. 1) a tube of tin Q q, four feet long, three inches wide, and weighing three pounds. II. To the lower extremity Q, I fufpended a plummet Q R; to the point A in the middle of the length of the conductor, I fufpended two very thin filver wires which I had ftraightened in the fire, that they might hang parallel to each other; in order to perceive their motions from a fufficient diftance, I faftened two bits of paper to their extremities, and fpread

a black

a black cloth on the wall, oppoſite to which I propoſed to watch them. III. I afterwards fixed a rule O P on a ſteady little ſtool placed at a great diſtance, that the rule might not diſturb the electricity of the apparatus; and I ſettled it in ſuch a manner, that it was parallel to a plane perpendicular to the axis of the conductor, which was horizontally placed; becauſe I knew that the wires muſt diverge in that plane. IV. Laſtly, I placed three ſmall cubes of lead within holes made in the rule O P, from which ſprung three very ſharp needles, which were to ſerve me as ſights.

854. Things being thus ordered, I fixed a ſight in L; ſo that my viſual ray that paſſed through both L and the plummet Q R, reached the extremity B of the wires, which hung parallel to the vertical A B: then a man inſulated and electrified from the Chain (or from the Machine) touched a metal leaf placed in *q* with a braſs rod (if he had touched the conductor itſelf, he might have moved it) and thus electrified the apparatus. This done, by moving the cube of lead along the rule, I placed another ſight N in ſuch a ſituation, that my viſual ray which went through the point N and the plummet Q R, hit exactly the bit of paper which then had roſe to D; this done, I bade another man, who held a tube S exactly equal to the former with a ſtick of ſealing wax, to touch the metallic ſheet Q with it, and thus take away juſt a half part of the electricity diffuſed in the tube Q *q*: in this very inſtant I placed another ſight M in ſuch a manner that the viſual ray that paſſed through the point M and the plummet, hit the point C to which the bit of paper annexed to the wire was now lowered, from D.

855. Having aſcertained, by repeating ſeveral times the operation, that the ſights were placed right; I ſuſpended another plummet A I to the point from which the wires hung; I meaſured the horizontal diſtances, L R and *r* I; I took the diſtances L M, L N; and then ſaying, as L *r* is to *r* I, ſo L N is to I G, and ſo L M to I H. I found I G, I H equal to the right ſinuſes of the ſemiangles of divergence D A B, C A B; whence I immediately knew the value of the chords of the ſaid ſemiangles. Having repeated this experiment ſeveral times in one day, and many times more afterwards; I always found the chord of the angle D A B

double

double to that of the angle C A B ; the errors were never more than a few hundredth parts of a line, and entirely vaniſhed when I compounded together the errors that had taken place in a great number of repeated experiments.

856. Since therefore, the chord of the ſemiangle D A B, which proceeds from the total electricity, is double to the chord of the ſemiangle C A B, which proceeds from the half electricity; and ſince, beſides the forces that ſuſpend bodies in different arches of a circle, are to each other in the ſame ratio as the arches are to each other; it follows, that the force of an electricity conſiſting of a double exceſs, is double to that of an electricity conſiſting of a ſimple exceſs.

C H A P. II.

On the motions of deferent bodies towards each other; on the compoſition of ſuch motions, with their motions from each other; and on the alterations that ariſe in theſe motions from differences in the maſſes of the bodies.

857. FROM the very beginning of this book (num. 6) I obſerved that the motions by which different bodies recede from each other, were motions *of a ſimple preſſion*; by which I meant that the electricity did not diffuſe itſelf away, either from the one or from the other ſyſtem: therefore, any ſuch motions of bodies towards each other as only take place from diſtances too great to allow of a real diffuſion of the actuating electricity, muſt alſo be looked upon as motions *of ſimple preſſion*; theſe motions I propoſe to examine in this chapter. A metallic wire or thread, ſomewhat heavy, adapted to the loweſt point of the conductor Y (Pl. I. fig. 1) deviates from the vertical line towards a wall that remains diſtant from it, two, three, or more feet, though no diffuſion of the electricity of the conductor into the wall takes place. Reciprocally, a ſimilar thread that hangs cloſe to a wall, deviates a little from it towards the conductor, though it remains at ſuch

ſuch a diſtance, that it certainly draws no electricity from it; this may be proved by the former experiment of the *ſcrutator*, or electroſcope (Pl. VII. fig. 1) which, being preſented laterally and underneath to the conductor, at the diſtance for inſtance of one foot, always directed its threads in the ſame manner, towards the ſame part of the conductor.

858. Now theſe approaches of bodies, ariſing from a ſimple *preſſion*, are ſometimes compounded with receſſions of the ſame; and they mutually modify each other. The two threads of the ſcrutator immerſed in the atmoſphere of the conductor, recede from each other, becauſe both become ſimilarly electrified, which is, by deficiency, as their natural fire is driven away by this atmoſphere; but in the mean while they alſo tend to move towards the conductor, which circumſtance leſſens the degree of their mutual receſſions.

859. As the tendency of two bodies towards the ſame point leſſens their divergence, ſo their tendencies towards different oppoſite places, increaſes their divergence.

860. If only one of the two bodies is attracted outwards by another ſtrange body, they will then unequally diverge from each other; and the angle contained between a vertical line drawn from the common point of ſuſpenſion, and the thread that is attracted, will be greater than the angle contained between the ſame vertical line, and the thread that is not attracted.

861. With theſe ſeveral motions muſt alſo be compounded the accidents ariſing from the different gravities of the bodies with which the experiments are made; and the divergences will be leſs according as the weight of the electrified ſuſpended bodies will be greater.

862. The gravity of bodies alſo alters their mutual approaches or receſſions. Let ſix threads be annexed to ſix equidiſtant points of a ſection X of the conductor X horizontally ſituated; the electricity will tend to make theſe threads, or hairs, diverge at equal angles from each other; and, in fact, if they are exceedingly fine, they will do ſo; but if their weight is ſomewhat conſiderable, the vertical thread, &c. which, in order to riſe, muſt ſurmount the whole force of its gravity, can no longer effect it, and

and will bend itſelf downwards; the horizontal threads will alſo meet with a reſiſtance that will hinder them from riſing, but will be leſs than that oppoſed to the former thread; and the two oblique inferior threads will indeed meet alſo with a reſiſtance from the increaſe of their weight, but which will be ſtill leſs than that met by the two horizontal threads.

863. If two threads are of different weights, it is plain that their mutual receſſion will be unequally modified by their reſpective gravities; and it is eaſy to determine what will be the degree and the law of ſuch modifications.

864. I might ſay ſomething with regard to the accidents that may cauſe the electrical motions of bodies, to vary, and which ariſe from differences in the latter's ſurfaces, ſhapes, and dimenſions; but I am afraid I have already too much expatiated on a ſubject, which, in many caſes may be better underſtood and developed by the obſerver himſelf, than deſcribed by the pen, however exact, of a writer.

CHAP. III.

On the motions of actual diffuſion in the electric fire.

865. I Preſent my hand ſideways to a flaxen thread, hanging from the conductor Y (Tab. I. fig. I): this thread, when my hand is at a certain diſtance, runs to it, clings to it, and diffuſes into it the exceſſive fire of the conductor. II. Alſo, from the ground, I preſent a flaxen thread to the conductor, and it runs to it, clings to it, and tranſmits the fire from it to my body, and thence to the ground. III. Or I preſent the ſame thread, to the thread that hangs to the conductor; when both run to each other, and the latter likewiſe diffuſes its fire into the former. IV. If the electricity be continued, then the threads part from each other, either becauſe they are but little deferent, or becauſe they are too much ſo. If the threads are of flax, and extremely dry, they join with more difficulty; becauſe the atmoſphere of the conductor more difficultly introduces a contrary electricity into that

thread which belongs to the ground: after the threads have joined together, they alſo preſerve their electricity ſomewhat longer, and they part from each other only for ſuch ſhort times as are neceſſary to enable both the electricity which is retarded in the thread belonging to the ground, to diſſipate, and a contrary one again to ariſe in it. If the threads are extremely wet, or which is better, if they are ſilver wires extremely thin, ſuch as are uſed by Aſtronomers for ſuſpending their pendulums, they join together, but ſoon part, and then unite again: this is becauſe ſuch wires, when joining and touching each other, inſtantly diffuſe away the whole electricity of the conductor; then they fall back, and again join together, when another exceſs has been accumulated.

866. Whence it appears, that the above junctions of threads, wires, or other bodies, do not continue but ſo long as the electric fire continues entirely to diffuſe itſelf away through them. This ſame principle alſo ſerves to explain other more complicated motions, which from their ſucceſſive alternations, may be called, electric oſcillations; the only difference is, that in theſe oſcillations, the approaching and receding motions by which they are formed, are complicated together; the following is an experiment very uſeful to render ſuch oſcillations conſpicuous. With an whole rolled ſheet of gilt paper I make a pendulum B, (Pl. V. fig. 4) which by a ſilk thread hangs from the cieling of the room, and remains at a few inches diſtance from the conductor D. If there is no ſtrange body near the ſaid pendulum, and the ſilk thread accurately inſulates it, the electricity of the conductor, however intenſe it may be, cannot make the pendulum move to the conductor; but as ſoon as I bring a ſtrange body near the pendulum, the latter flies to this body, and gives it a ſmall ſpark; it then flies back to the conductor and receives from it a ſtill larger ſpark; this ſpark the pendulum carries again to the ſtrange body, and thus continues to oſcillate; that is to ſay, the pendulum B does not move towards the conductor, but when the latter is enabled to actuate in it an electricity contrary to its own; and this it cannot do until another body is brought near the pendulum, into which the conductor may drive the natural fire of the latter.

867.

867. Sometimes, inftead of a folid body, I carry the flame of a candle near the pendulum; and this flame, even at the diftance of two feet, caufes the pendulum to fly to the conductor, then to part from it for a fhort while, and invifibly diffufe away the fire it has received from it, then to fly again to it, there to receive another fpark, and thus to continue to ftrike againft it, like a battering ram driven by an invifible force. With this latter very fimple experiment, I confirm the truth of the principle that produces and governs electric ofcillations, which is nothing more than a diffufion of the electric fire effected in a certain manner; and I moreover fhew how the air becomes apt to favour fuch diffufion, firft, by virtue of the rarity introduced into it by the flame, and then by that of the deferent vapours that remain fpread within it, which the electric fire, while diffufing itfelf from the pendulum, gathers and difpofes in its way.

868. The fig. 5. of the Pl. V. reprefents an experiment in which the diffufion of the electric fire, by means of an ofcillation, is rendered ftill more manifeft. B is a fquare piece of pafteboard lined with gilt paper, the lateral angles of which are fomewhat blunt; A B, is a long filk thread which infulates it in the middle between the conductor D, and a ftrange body C, the furface of which is pretty broad: this pendulum ofcillates between thefe two bodies, exhibiting a ftar on that of its fharp angles which is directed towards the ftrange body, and a brufh on that fharp angle which ftands neareft to the conductor, which in this cafe is fuppofed to be electric by deficiency.

869. Univerfally, the ofcillations that are produced by an electric force, differ from thofe produced by gravity, in this; in the latter cafe, the motion is accelerated as the ofcillating body moves towards the loweft point; but in the former, this acceleration is complicated with another acceleration towards the fyftems contrarily electric between which fuch ofcillations take place, and is caufed by the fire which diffufes itfelf from the one to the other.

870. If inftead of gilt paper, a ball of metal be ufed, which hangs between two metal bells, the one of which is annexed to an electrified fyftem; the ball will tranfmit the electricity of the one

ſyſtem or bell, to the other, by ſtriking againſt both alternately: balls or bells thus diſpoſed are the means uſed by philoſophers to be informed of the ſpontaneous motions of the atmoſpheric electricity.

871. If a ſeries of ſuch bells be properly framed and diſpoſed, the firſt and laſt of which communicate with different ſyſtems; the balls inſulated between them may thus be brought to form among themſelves a kind of harmony.

872. If the ſmall bells be diſpoſed with regard to their tones, after the ſame manner as the ſtrings of a harpſichord, and communicate with each other by means of inſulated ſmall inſtruments like the keys of a harpſichord, ſuch bells only will ſound as ſhall have theſe keys removed from them, and thus a kind of electric muſic may be obtained, and an electric harpſichord effected: to this add, that care muſt be had to place two different keys between each pair of bells, that the oſcillating ball may draw a ſound from each of them ſeparately.

873. Theſe electric oſcillations may ſerve for a number of entertaining experiments; ſuch for inſtance, is the *ſpider*, contrived by Dr. Franklin; ſuch is the *ſwing* which I mentioned in the ſecond chapter of *Artif. Elec.* —A B (Pl.X. fig. 2) is a long thin tube of glaſs, to which two needles are adapted in B, which ſerve as an axis to it; A and C are two gilt figures which muſt be without any ſharp angles; the whole is placed between four balls, two of which D, d, communicate with the Chain, and the other two E, e, with the Machine; and between theſe balls, the above figures will continue to play conformably to the principles above explained.

874. Theſe kinds of inſtruments or apparatus, are rather plays to amuſe, than experiments to inform; and at the ſame time, there are other operations which are ſo complicated, that they rather ſerve to tire and perplex the obſerver, than to afford him any real inſtruction: ſuch are experiments made by electrifying a number of pieces of metal leaves, and making them oſcillate at once; ſuch are alſo the motions that take place between particles of ſawings of wood, which experiment has been deſcribed by the Abbé Nollet to the Academy of Paris; but as this experiment, as well as others

of

of a complicated kind, depend on the principles above defcribed, any particular and minute explanation of them would be unnecef-fary.

875. I might alfo, in order to render the repulfion arifing from homologous electricities ftill more manifeft, have introduced the inftance of a jet of water, produced by the water's own electricity, and alfo that of fuch a jet, produced by the prefence of an electrified fyftem only. If you prefent under a conductor, a thin jet of water, which fprings upwards, you fhall fee it divided into a number of fmall drops; and while thofe in the middle will rife to the conductor through a very crooked way, the lateral ones will be driven from it.

876. And not only the combined motions of a number of corpufcles at once, but the fimple motion of a fingle corpufcle placed between two fyftems, will alfo exhibit confiderable deviations and apparent irregularities; and which are fuch that the ufual characteriftics and laws of electric ofcillations can fometimes fcarcely be acknowledged in them.

877. I fhall here defcribe one of thefe irregularities, which takes place in the apparatus reprefented in fig. 9. Pl. X. Z P, is a filk hair five feet long; I ufe it of fuch length, to that end that the ofcillations may be more flow and confpicuous; to this hair a gilt ball is annexed about two lines and an half in diameter, and weighing only two grains. This ball is difpofed in the middle between two balls of brafs, A, B, which rife from the belly of two bottles, *a*, *b*, and are half an inch in diameter: both bottles lie on the ground; *a* is charged, the other not, and moreover, its lining and coating communicate together.

878. I. When the bottle *a*, has only a fmall remnant of charge left, the irregularities of the ofcillations become ftill more difcernible; the fmall gilt ball P, from the bottle A, ufually rifes obliquely, and moves along the oblique line A *m*, it goes beyond the ball B, and afterwards fall back on it: from the latter it is repercuffed with a much lefs force than it came, and moving through a line B*n*—A, ftraiter than before, turns back to the ball A, without going beyond it. The caufe of the lateral deviation of the ball P, lies in the difficulty which this ball P, finds to ftrike

againft

againſt the two inner points of the balls A and B; in which caſe it would be directly reflected from the one to the other. In fact, though it be very rare, yet it ſometimes happens that the pendulum P is directly carried againſt theſe inner points; in ſuch caſe it moves directly from the one ball to the other, and thus performs two, three, or more oſcillations. But if to the balls A and B, two plain parallel metallic ſurfaces are ſubſtituted, then the pendulum P regularly oſcillates between them without any irregularity or deviation. To the conſiderations of both the oblique ſtroke and reflection that takes place when balls are uſed, this other muſt be added, viz. that the pendulum A is repelled from the ball A in proportion to the latter's greater electricity, of which it now partakes: the ball B, which communicates with the ground, is only actuated by the ſlight degree of deficiency which the ball A can produce in it, by virtue of its own atmoſphere; and therefore the pendulum P is repelled by it with a force proportionably leſs.

879. II. While the bottle *a* continues to be animated with a pretty ſtrong electricity, the whirling motion exhibited by the pendulum P, after it has touched the ball A, becomes extremely varied, according to the numberleſs combinations that may ariſe between the different circumſtances which may influence the oſcillations of this pendulum: ſuch are, I. The particular place of the pendulum on which it is ſtruck by the ball A. II. The angle under which it is repelled. III. The degree of force with which it is repelled. IV. The effect of the weight or gravity of the pendulum. V. The action of the atmoſphere of A, which continues to repel that pendulum till it has touched the ball B, &c.

880. Between oſcillatory electric motions, and the electric motions produced by *ſuſpenſion*, there is a great affinity. Out of a ſilver leaf, I cut a parellelogram B (Pl. V. fig. 6) the angles C, D, of which are very ſharp; I ſuſpend it between two metallic plain ſurfaces, connected with the hooks of two bottles that are both charged, but after a contrary manner; or if the one be not charged, there muſt be a communication between its coating and its lining. I. When there only remains a very ſmall electricity in the bottles, then the leaf oſcillates from the one ſurface to the other, alternately ſtriking againſt them. II. While the electricity continues to be very

very ſtrong, the leaf B remains ſuſpended between the two bottles, only performing very ſhort vibrations, though the diſtance between the bottles is greater than the intervals to which the points extend their wind; if this diſtance between the bottles be leſs than the diſtance to which the points C, D, can drive the electrified air, then the leaf B remains unmoved. III. If the angles C D, are unequal, then the leaf B remains ſuſpended at diſtances from each bottle proportioned to the different degrees of ſharpneſs of its angles. IV. If an angle be very ſharp, and the other entirely rounded, then the latter turns itſelf towards the electrified bottle, and remains united to it, as Dr. Franklin has very well obſerved, while the other angle remains directed outwards, however diſtant the other bottle may be. When I make this experiment, I uſually ſuſpend the leaf with a thin ſilk hair, in order to diſcern the better its motions. As for the laws of theſe kinds of motions, they are conſtantly the ſame, ſo long as the leaves are preſented immediately to an electrified ſyſtem: but it muſt be obſerved, that after whatever manner the experiment is made, the leaf, every other circumſtances being equal, remains neareſt to that ſyſtem which is not electrified; and the reaſon of this is, that, as I obſerved before, the ſyſtem which is not electrified determines with leſs force the current of air which tranſmits the electricity, and makes it ſpread itſelf to a leſs diſtance than the electrified ſyſtem.

881. But what elſe are all the different kinds of electric motions which I have hitherto deſcribed, but ſo many particular conſequences of the general law, that the electric fire drives and diſpoſes in its way ſuch deferent bodies as may favour its paſſage from one ſyſtem to another? I. Deferent bodies, when they are without any ſharp angles, cannot give or receive electric fire but as long as they are in actual contact with other bodies; they are therefore neceſſitated to oſcillate between ſuch bodies or ſyſtems as are placed too far for them to touch them at the ſame time, and alternately to ſtrike againſt them. II. If the exceſs of the one ſyſtem above the other be very ſmall, it cannot then excite any conſiderable electric wind, and even ſharp bodies are, in ſuch caſes, obliged to oſcillate, and alternately to touch both ſyſtems.

III. If

III. If the relative excefs be fomewhat ftronger, fo that it may excite fome pretty confiderable electric wind from the fuspended body, though fuch wind does not extend to both fyftems, then, the ofcillations are fhortened, the metallic fheet ofcillates through fuch tracts only as are neceffary to enable the electric wind that arifes from its fharp angles, to reach both fyftems, alternately. V. If the relative excefs be confiderable, then, as the one of the points of the deferent leaf throws the air deprived of its own natural fire to a great diftance while the other point likewife throws the air loaded with exceffive fire to a fufficient diftance, the leaf remains unmoved.

882. But the law concerning motions of an actual diffufion (9) viz. *that the electric fire difpofes deferent bodies in its way, that it may diffufe itfelf through them with as much celerity as both its own force, and their deference can allow,* this law, I fay, becomes particularly manifeft, when we confider the arrangement and fucceffive connection of a feries of corpufcles lying between two fyftems: which arrangement of corpufcles, I have ufually expreffed by the words, a *difpofition of the fame in the fire's way.* Of this kind of motion I have given feveral inftances, and a number of others might be added. Moift vapours that fpring from metallic eolipiles contrarily electrified, and the fteams of which are obliquely directed relatively to each other, bend themfelves towards each other, unite together into one continued ftream or feries of particles, and by this means the fire which fuperabounds in the one, is communicated to the other.

883. But with regard to this difpofition of deferent bodies in a continued feries, this other law muft be laid down, viz. *a feries of corpufcles is not formed by the electric fire, nor fubfifts, but as far as this fire finds no eafier way to transfufe itfelf.*

884. The duft of bran which is prefented underneath to a conductor, forms conducting ftrings of its own particles, only when the electricity of the conductor is weak, becaufe it then finds a fufficient paffage through fuch particles as are but little deferent: if the electricity be ftrengthened, then the ftrings break; clouds, as it were, of feparate particles of bran rife, which by means of their numbers, and of the rapidity of their motions, conduct

duct more fire to the neighbouring body, than the above ſtrings were able to perform.

885. If the intenſity is farther increaſed, then another kind of diſpoſition of bodies *in the fire's way* takes place, which I may call a violent one: the ſparkling element then excites moſt active effluvia from the deferent particles of the water, for inſtance, which it finds in its way, and by their means explodes the air away; by this means the electric fire moſt copiouſly and eaſily diffuſes itſelf.

886. To the many examples which I have formerly mentioned of violent diſpoſitions of bodies in the fire's way, I ſhall only add a compariſon between it, and another phenomenon of the ſame kind, which is effected in a milder manner. Make a large drop of water hang from the rounded extremity of a rod of metal, annexed to a conductor: as you will excite the electricity of the conductor, the drop will ſtretch itſelf towards the ſurface of the water that riſes from the edge of a glaſs, full to the brim, placed under it, which it even ſometimes attracts; and by ſucceſſively lengthening and ſhortening itſelf, according as the electricity which it endeavours to transfuſe will be more or leſs intenſe, this drop will exhibit a number of various accidents. But if, through the ſame drop, a pretty conſiderable battery be diſcharged, then the vehement ſpark will explode the greateſt part of it into vapour, and through this vapour, ruſh united and compact.

CHAP. IV.

On the motion of bodies immerſed in an electrified medium.

887. IN my ſeventh letter on Terreſtrial Atmoſpheric Electricity, I wrote very minutely on the electricity which is raiſed in the air of a room. In the fifteenth letter, p. 343. I gave an account of the electricity introduced into an extenſive vaulted room: but it was only after writing the ſaid book, that I perceived that the electricity did not really diffuſe itſelf through the ſubſtance itſelf of the air in the room, but through the vapours only

with which it was impregnated. I at firſt experimented in a narrow room, where a fire was kept burning in a *Pruſſian* chimney; but even when there was no fire in the room, I could electrify it, provided I had remained a while in it, with the windows and door ſhut. In the large vaulted room, I kept a fire in an iron ſtove, but though the air remained limpid and tranſparent, yet I was afterwards obliged to acknowledge that the oblique iron pipe adapted to the ſtove, had not entirely diſcharged the vapours of the fire, and that to their remnant, though imperceptible, was owing the electricity which I was able to excite. In fact, when I afterwards experimented immediately after ſhutting the windows, and in very dry weather, I could not by any means, excite any electricity in the air of the room.

883. Thence I had recourſe to the expedient of raiſing, whatever might be the ſtate of the weather, vapours that might favour my experiments. In my little book on *Electric Atmoſpheres*, which I ſent to the Royal Society on the 26th of February, 1769, I related in the numb. 78, how, by burning frankincenſe, I raiſed a vapour which very expeditiouſly and viſibly produced the ſame effects as the vapours which in the night had riſen from my body.

889. In experimenting in air thus prepared, I obſerved, I. That fine flaxen threads annexed to the conductor, ſoon acquired their greateſt divergence; but if I continued to excite the electricity, this divergence leſſened very ſenſibly. This decreaſe of divergence took place with a kind of regular gradation, eſpecially when a coated ſheet of glaſs was charging: ſo long as the electric fire, was employed, as it was at firſt, in forming the charge, it gradually carried the threads to a pretty conſiderable divergence; but as the electricity grew more intenſe, then the fire began to diffuſe itſelf out of the ſheet, into the deferent vapours ſpread around it, and it thereby counteracted the electricity of the threads, and ſoon leſſened their divergence.

890. When afterwards the electricity began to be ſpread to a certain diſtance in the vaporous air, if I touched the conductor, I obſerved that the threads annexed to it, vibrated in order to join together; but before they actually effected it, they rapidly moved

again

again from each other. In order more accurately to inveſtigate the cauſe of theſe accidents, I excited afreſh the electricity of the conductor; then with a flaxen thread very dry, that is little deferent, I touched the conductor at ſeveral ſucceſſive times; and I obſerved that, at every touch, the divergence of the threads leſſened, ſo that they at laſt reſumed their natural ſtate: if I then ſtill continued to touch the conductor, a new divergence aroſe in the threads. I repeated the experiment, and when the threads were brought to diverge in conſequence of their own electricity, I directed a metallic point to the conductor, and as I brought it nearer to it, the threads firſt recovered their natural ſtate, then diverged afreſh. When, after the threads had recovered their natural ſtate, whatever might have been the manner of effecting it, I preſented my finger to the conductor, I received a ſpark from it, and a new divergence inſtantly took place. I charged a coated bottle, ſupplied with a ball in A (Pl. X. fig. 9); I preſented this ball to the threads of the conductor, which were in the ſtate of their firſt divergence, and they flew from it: I preſented the ball to the ſame threads, when in the ſtate of their ſecond divergence, and they flew to it; and in this ſtate, as I moreover obſerved, they flew from the belly of the charged bottle which I preſented to them, holding it then by the ball A.

891. From the combination of all theſe facts it follows, I. That in the caſe of the firſt divergence, the threads of the conductor did not diverge, but in proportion to the exceſs of their own fire, above the fire that was diffuſed in the air. II. When the reſiduous fire on the ſurface of the threads was brought to an equality with the fire in the vaporous air, then the threads hung in their natural ſtate: which is the caſe with all bodies immerſed in a medium equally electrified with themſelves. III. The two portions of exceſſive fire, that which was diffuſed in the conductor, and that in the ambient air, endeavoured not only to deſtroy each other, but alſo to drive away the natural fire in them: ſo that it happened that the conductor drove the fire diffuſed in the vapours contiguous to it, into continually more remote ones; and reciprocally, when a ſtrange body communicated with the conductor, then the exceſſive fire that was diffuſed in the air, began to drive

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from the conductor not only the exceſſive fire in it, but alſo a proportional part of its natural one.

892. The latter propoſition may alſo be demonſtrated by a very ſimple experiment. With one of your hands, touch the conductor, and preſent a finger of the other hand to the threads; they will fly from it. Place your finger between both, they will likewiſe fly from it, increaſing their reciprocal divergence: if you preſent your finger externally to one of them, they will both recede from it: that is to ſay, the exceſſive electricity diffuſed in the air, will drive the natural fire from your finger, as well as from the threads.

893. The nature and effects of the electricity diffuſed in vaporous air, is rendered conſpicuous by the different motions of corpuſcles contained in it, which communicate with the ground. Hold two threads between your fingers, and with theſe walk about the room, after a pretty copious electricity has been diffuſed into the air of it; and you will ſee them in any part of the room diverge from each other, as well as from the tables, the chairs, the walls, or any body in the room to which they may be preſented: if to the tables, chairs, &c. you have previouſly annexed threads, you will ſee the latter diverge, as ſoon as the electricity begins to diffuſe itſelf, and continue to do ſo, ſo long as there remains any.

894. I muſt not omit here a few obſervations I have made, with reſpect, firſt to the propagation, then to the duration of this electricity. It does not diffuſe itſelf at once from the conductor, but ſucceſſively reaches parts more and more remote from it; ſo that if, after only a ſhort exciting of the electricity in the room, the friction is ſtopped, the threads I hold in my fingers, diverge only at the diſtance of a few feet from the conductor. In order to have the threads diverge at a greater diſtance from the conductor, the friction muſt be continued longer: and in the large vaulted room which I mentioned above, I found that threads, carried to the diſtance of twenty-four feet from the conductor, did not begin to diverge but after the excitation had been continued ten or twelve minutes.

895. It is of courſe to be concluded, that the divergence of threads, is leſs at a great diſtance from the conductor, than near it.

896.

896. With regard to the duration of the electricity diffufed in air, it lafts longer according as the vapours or effluvia through which it is diffufed, are rarer and lefs deferent; therefore, the duration of the electricity is longer in proportion as a longer time has been employed in exciting it. It has often happened to me, when returning into a room from which I had been abfent an hour and an half, to find that the threads annexed to a conductor that communicated with the ground, continued to diverge; but then the air in the room was only impregnated with very rare, and as I think, imperfectly deferent vapours, which had efcaped out of the ftove. At other times, I rendered the air vaporous with two eolipiles; when the electricity raifed in it lafted, I found, much lefs than in the former inftance. But when I burnt frankincenfe, the electricity that was diffufed through its fmoke, lafted but little: the reafon was, that, though the particles of frankincenfe might be of a pretty much infulating nature, yet they rofe very thick, and diffufed themfelves with fuch a copioufnefs as to create in the room a very fenfible opacous fog.

897. I fhall relate here a very fimple method of experimenting on the electricity that takes place in vaporous air, and inveftigating its laws and different accidents. Let the fig. 3. Pl. V. reprefent a glafs bell C, fufficiently ample, fupplied with a rod A B, to which two very thin flaxen threads are annexed; and if the weather be fomewhat damp, it will be fufficient to raife a fmall quantity of vapours under the bell. Place the bell on a fmall ftool; make the hook of the rod A B, communicate with a conductor; and after a fhort excitation of the electricity, the air under the bell will be competently electrified. If then you touch the hook A, the two threads B will begin to move towards each other, in confequence of their own electricity; but as this electricity will foon ceafe, they will pafs to a fecond divergence in confequence of the *vaporous* electricity in the room, which will drive the natural fire both from them, and from the rod A B. If I excite again the electricity in the conductor, the threads will again begin to move to each other, in confequence of their own electricity, which is equal to that in the air; but they foon will again diverge, becaufe their electricity will foon prevail over that of the latter, &c. Experiments

periments of this kind may very well, as the reader may ſee, be made under a glaſs bell, and by this means, there is no occaſion for darkening the whole room with vapours.

898. If after introducing a vaporous electricity into the bell, you proceed to draw the air out of it, very remarkable accidents will take place; but of theſe, as well as of the electricity of the air and vapours under the bell, I propoſe to treat in the next chapter.

C H A P. V.

In which electrical motions performed in dilated air, are examined.

899. IN the ſummer of the year 1752, in which I wrote my book on *Natural and Artificial Electricities*, ſeveral circumſtances hindered me from examining with a ſufficient degree of attention, the electrical motions that took place within the vacuum of an air pump. The chief obſtacle I met with at that time, was the inconveniency of my air pump, as well as the imperfection of ſome of the inſtruments that belonged to it. But having ſince got an air pump to be made in which the defects of the former were corrected, I ſhall here relate a few of the obſervations I have made with it, (Pl. VII. fig. 11, 12, 13).

900. Uſing jointly with my new air pump, the electric apparatus repreſented in fig. 9. Pl. II. I made the following experiment. A thin ſilk hair, four inches and an half long, was faſtened with ſoft wax to the upper part of the inſide of a glaſs bell; to it was ſuſpended the pendulum P, made of tinfoil, weighing rather leſs than a grain, and diſtant two lines from the ball B, which received its electricity from the Chain, and only half a line from the ball C, which, through the rod CD, diffuſed this electricity into the ground: the barometer, at the time of the experiment, was 27 inches, nine lines high (Italian meaſure), and here follows what I obſerved.

901. I. When only an inch of air (that is a $\frac{1}{28}$ nearly, of that contained in the bell) remained in it; the ſmall electric pendulum

very

very quickly oscillated between the balls B, C, and smartly struck against them. II. When only 9 lines of air remained in the glass bell, the pendulum still moved with great velocity from one ball to the other, so that it was very difficult to count the oscillations; though it must be observed, it then struck against the balls with less force. III. When there remained only about six lines of air in the bell, the pendulum continued to strike against the ball C, but seldom against the ball B. IV. When only 5 lines of air remained in the bell, the pendulum still continued to strike against the ball C, but could only a few times reach the ball B. V. When only four lines of air remained, the pendulum continued to touch the ball C, but reached no longer the ball B. VI. When three lines of air remained, the pendulum now and then reached the ball B, but always kept an whole half line distant from the ball C. VII. When only a line and an half remained, the pendulum still continued to reach the ball B C, but, when falling from it, remained an whole line distant from the ball B.

902. From this experiment it plainly appears, that when the air is much dilated, so that there remain under the bell only nine lines of air, that is $\frac{1}{36}$ part of the atmospheric air; the electric signs seem, at first sight, to have lost nothing of their former quickness.

903. But when afterwards the air becomes dilated to a greater degree, the electric signs under the bell, begin to grow proportionably languid.

904. Yet, even the electric signs that take place in the air, when first reduced to a $\frac{1}{36}$ part of the atmospheric air, are found, if considered with attention, to be less vehement and less rapid, than those performed in air, either condensated, or that retains its natural density. I say, they are less vehement, because in air that retains its usual density, the pendulum, especially if it be somewhat distant from the balls; (Pl. V. fig. 10, 11.) performs around them whirling motions, such as those described in num. 878, 879; the chief cause of which is the intensity and extension of the atmosphere spread around the ball, which communicates with the Chain. When the air begins to be pretty much dilated, so that the fire begins to meet with a free passage, the atmosphere around the

the ball B, becomes lefs intenfe and lefs extended, and the electric motions become of courfe lefs complicated; in fuch cafe, the pendulum runs from the ball B to the ball C, for inftance, through the curve line *y*, *x*, *u*, *t*; it ftrikes the ball C on its back part *t*, where the atmofphere of the ball becomes lefs intenfe, and turns back to the ball B, through the curve line *t*, *s*, *r*, *q*. But even in air that retains its denfity, fuch motions may be brought to a great degree of fimplicity and regularity. Leave off exciting the electricity, and as foon as it will fubfide, the pendulum will ofcillate from one ball to the other, without any deviation; this is, becaufe the intenfity and extenfion of the electric atmofphere will then leffen together with the electricity of the conductor.

905. But to conclude this fubject of motions that take place in air extremely dilated, it remains to be faid, in the fourth place, that fuch motions happen chiefly when the ftream of fire is very weak: for inftance, I faid that when there remains only one line and an half of air in the glafs bell, the electric pendulum continues to reach the ball B, and that when it falls back, it remains one line diftant from the ball C; now this motion, which is the greateft that takes place in air brought to fuch a rarity, does not take place when the glafs is excited to the utmoft, and an uniform, fteady, red, violaceous ray takes place between the two brafs balls within the bell; it takes place when the friction of the glafs, is either very weak, or when it begins or ceafes; which fact, however paradoxical it may appear at firft fight, is no lefs conformable to reafon than to truth. The electric fire does not fet bodies in motion, in confequence of its furrounding them on all fides, and involving them in a continued ftream; but it fets them in motion in confequence of its acting with unequal force on oppofite fides of them; which is not the cafe in the above experiment, except when the electric ray or ftream, either only reaches the pendulum, and does not yet proceed farther; or when it parts from it, without being fucceeded by another electric effluvium.

906. From the fame principle may alfo be underftood the reafon of another experiment made in dilated air, with minute particles of gold leaf, or other corpufcles of a like kind. On a brafs difh (Pl. V. fig. 9) placed on the bafon of the air pump, I

fpread

ſpread a few ſuch corpuſcles: when the vacuum is made, I lower the metallic rod, which is terminated in a kind of ring, ſo as to bring this ring to a certain diſtance from the corpuſcles: when the electricity begins to be excited, ſome few corpuſcles riſe to the ring, but if the electricity is then farther excited, the other corpuſcles remain unmoved. The reaſon is, as I ſaid above, that the beginning effluvium, alone, of electric fire, can move the corpuſcles, not a continuous, uniformly diffuſed ſtream of it. The motions of theſe corpuſcles, indeed appear very quick, and might make us think at firſt, that the preſence of the air is little neceſſary to electrical motions; but a ſingle obſervation will convince us of the contrary: if the vacuum be brought to a line and an half of air, the motion above will only take place at a very ſmall diſtance, whereas in the open air, corpuſcles move at the diſtance of three, four, or even ſix feet from the conductor: from all this it therefore follows, that in proportion as air is more completely dilated, and the electric fire conſequently finds a freer paſſage through it, electric motions leſſen, grow languid, and fail. In the experiment of an electric pendulum placed between two balls, at the ſmall diſtance of about two lines, any ſmall increaſe of this diſtance will be ſufficient to prevent the pendulum from touching either ball any longer, and make it keep oſcillating between both with a great ſlowneſs, and through very ſhort arches.

907. In order to obtain as great light as poſſible on the ſubject, I have not omitted to experiment in the barometrical vacuum, as I have related in num. 46 of *Terr. Atm. Elec.* The difficulty I found to unite the glaſs melted with a lamp, around the threads of amianthus I tried to uſe, obliged me to inſert balls made of ſuch threads, into the upper part of the tubes blown into the ſhape of flaſks, (Pl. V. fig. 13.) after having well warmed them on burning charcoal. Theſe tubes I gradually filled with mercury, and ſucceſſively extracted the air by the help of an iron wire. Even in the beſt of the barometers I thus made, there muſt have been a remnant of air; ſince when I ſhook it in the dark, I could perceive a ſhort ring of electric light * which accompanied the circum-

* I mean to ſpeak of the little lucid ring produced by friction, which I have analyſed at the end of the chap. II. that the light of *communication* may appear in a complete

 vacuum,

ference of the mercurial cylinder; which luminous appearance I could not perceive in other barometers more completely emptied of air, which I ſucceeded to make when I inſerted no threads of amianthus into them. With the barometer made as deſcribed above, I experimented as follows. I. Into a tube of glaſs exactly equal and ſimilar to the tube of the barometer I uſed, I likewiſe inſerted a ball of threads of amianthus, which I made communicate with the ground through an iron wire; the mercury of the veſſel into which the tube of the barometer was dipped, communicated alſo with the ground. Making choice of a very dry day, I inſulated myſelf, and communicated with the Chain. When I preſented my finger to the top of the barometer, a ſmall ſpark leaped to it, and in the ſame inſtant the whole empty ſpace of the barometer, (*Terr. Atm. Elec.* p. 48) was filled with an electric light uniformly diffuſed, of a red purpureous colour, which was leſs vivid than the light of the ſpark, in proportion as it was more extenſively diffuſed: but while this took place, I could hardly perceive a motion in the threads of amianthus. On the other hand, when I preſented after the ſame manner, my finger to the other tube, made of the ſame ſhape as the barometer, a ſpark no ſooner leaped from my finger to its external ſurface, than another leaped from the internal one, to the threads of amianthus; which ſpark often ſhook not only a few of the threads, but the whole aggregation of them. In ſhort, I aſcertained this truth, that even in very minute bodies, the electric motions grow very weak in proportion as the vacuum grows exact in which the experiment is made. I continued to experiment, and made bubbles of air enter into the barometer, and then obſerving in the dark, I ſaw them lighten as they were riſing: ſome of them were not unlike thoſe lightning globes which ſometimes are ſeen during *Auroræ Boreales*; but the circumſtance I chiefly propoſe to obſerve, is that the mercury in the barometer was ſcarcely lowered three inches, when the whole maſs of threads began to be agitated at every ſpark that was drawn.

vacuum, is what I never doubted, and I have even deſcribed a number of inſtances of ſuch light.

908.

908. Though I have hitherto only mentioned the vibrations and complicated motions of threads of amianthus; yet I have alſo examined in the vacuum motions of a ſtill ſimpler kind: here follow the laſt experiments I have made on that ſubject. I. In a glaſs bell nine inches high, four wide, and through the neck of which a braſs rod was inſerted, I ſuſpended two fine threads about ſixteen lines long, to the extremity of this rod, with balls of pith of elder hanging by them, and ſo light that they ſcarcely could ſtretch them. II. I eſtabliſhed a communication between the rod and the conductor, and after the glaſs had been rubbed a long while, there aroſe within the glaſs bell an electricity that made the threads begin to move towards each other when the conductor was touched; but they ſoon paſſed to their ſecond divergence, reſulting from an *ambient* electricity: if the conductor was again electrified, the threads again began to move towards each other, and afterwards diverged in conſequence of their own electricity. III. I then began to make a vacuum under the glaſs bell, and after the firſt ſtrokes of the embolus, when the uſual fogginefs began to appear within the top of the bell, it ſeemed as if the divergence of the threads in conſequence of the electricity in the ambient air, were ſomewhat increaſed; but this increaſe was but ſmall, and laſted but little: when I drew more air, the divergence proved ſtill leſs. However, though the air was afterwards almoſt completely extracted, ſo that little more than two lines of it remained within the bell, yet a divergence continued to take place between the threads, no leſs than fifteen or twenty degrees. IV. Having farther dilated the air, ſo that the remnant of air was now no more than two lines, I ſaw the electric fire flow from the threads to the baſon underneath, under the appearance of two continued rays: when theſe rays flowed with ſome intenſity, the threads immediately fell into a contact with each other, but reſumed ſome divergence whenever the rays ſeemed either to ceaſe or grow languid. V. Admitting afterwards the air under the glaſs bell, and mean while touching the Chain, I perceived the divergence of the threads increaſe; but it ſoon began to leſſen very ſenſibly. Though the air was now freely admitted into the bell, yet there was ſtill a ſmall remnant of divergence; and when after raiſing the bell from the

 baſon,

bafon, I prefented a brafs rod to the threads, they receded to a certain fhort diftance from it. VI. The above accidents are to be obferved, when the balls are placed pretty high in the vacuum, for inftance, at about five inches above the bafon: if the rod be lowered, then the balls, as they move downwards, lofe their divergence, fo that, when they are only an inch diftant from the bafon, they diverge no more, however ftrongly the electricity may be excited, and the fire is then feen continually flowing through the vacuum without moving them.

909. The latter circumftance feems to indicate that the electricity by virtue of which the balls diverged, arofe from fome fire, which from the metallic neck of the bell, ran along the external furface of the latter, and actuated an electric atmofphere from the correfponding internal furface; becaufe the experiment was made in good weather, and the bell was fmooth and free from vapours. However, when can air be faid to be completely pure? I. It might be, that the lower vapours retained little or no electricity in confequence of their being nearer the deferent bafon. II. The increafe of the divergence between the two balls of pith of elder, that took place when the air began to be drawing out, may have been owing to the vapours that gathered near the top of the bell, where they formed a kind of fog, and thus encreafed their own electricity, after the fame manner as we have feen that a man increafes his own electricity by gathering his limbs together on an infulating ftool, (num. 457). III. The little increafe of divergence that took place when the air began to be admitted, was perhaps owing to a like caufe; in that inftant the electrified vapours were driven towards the top of the glafs-bell. IV. In repeating this experiment, I have feveral times, both before extracting the air, and after, wiped the bell with a handkerchief, and the divergence ftill continued. V. The fame divergence happened feveral times to continue, though at the fame time a very fine thread which I prefented externally to the bell, was not attracted.

910. But whatever the caufe of this electricity might be, whether it arofe from the fire that flowed externally, or internally, along the furface of the glafs, or from the fire that actuated the vapours diffufed within the bell, and after drawing off the air,

ftill

ſtill continued in them, though they became much rarer, this at leaſt is certain, that the effects of this electricity were entirely ſimilar to thoſe that obtain in the *aereo-vaporous* electricity: and in the ſecond place, from theſe experiments it ſeems to reſult that the natural quantity of electric fire lies diffuſed in the vacuum, in the ſame manner as it does in other mediums or bodies; in fact, both the exceſſive fire which is actually accumulated in the vapours of the air, brought to the rarity of two lines, and the exceſſive tenſion which takes place in the fire of ſuch a medium, ſuppoſe the preſence of the natural quantity of it; nor does it ſeem that this natural quantity may be actually diffuſed in ſo rare a medium, without being likewiſe diffuſed in more complete vacuums. And here we muſt add, in the third place, that air reſiſts the electric fire, keeps it back, and compells it to accumulate, by making it coaleſce, not with the ſubſtance of other electric fire contained in it, but with its own ſubſtance. In the fourth place, with reſpect to our preſent enquiry concerning the mutual electric receſſions of bodies in dilated air, it appears that they are proportioned to the quantity of the electricity that is retained by the remaining air.

CHAP. VI.

The cauſes of electric motions.

911. WE are under no neceſſity of explaining electric motions by certain indeterminate kinds of attractions, or repulſions, as Sir Iſaac Newton has been obliged to do with reſpect to the phænomena of the univerſal gravity. No mechanic principle of gravity has yet been diſcovered by our ſenſes; and all thoſe which men have hitherto imagined in order to explain this phænomenon, have been found inſufficient, nay repugnant to the evidence of facts: but in regard to electric motions, we manifeſtly perceive them to be owing to a moſt active element, to the *inequilibration* of which electric motions exactly correſpond, with reſpect both

to

to their exiſtence, and to their quantity and quality. I ſay with reſpect to their *exiſtence*, becauſe electric motions never take place but when the electric fire is unbalanced, and actually endeavours to recover its equilibrium: I alſo ſay, with reſpect to their *quantity*, becauſe they are always found to be proportioned to the greatneſs either of ſuch unbalancement, or of the diffuſion of it that takes place; and alſo to their *quality*, becauſe the ſame kinds of motion are always ſeen to take place between bodies contrarily electrified, and likewiſe between bodies ſimilarly electrified. If to theſe conſiderations we add that of the electric atmoſpheres, that is to ſay, if we reflect that from the unbalancement of the electric fire in deferent bodies, an adequate unbalancement always ariſe in the fire belonging to the ambient air, whence ariſe motions of *ſimple preſſion* in thoſe intervals through which the electric fire cannot actually flow, and ſometimes alſo of *actual diffuſion*, it will, I ſay, appear evident *that between the actual unbalancement of the electric fire, or a diffuſion of the ſame, and the motions of bodies, there is a connection of both cauſes and effects.*

912. The diſcovery of ſuch a connection invites us to ſeek for ſome more preciſe operation or action of the electric fire, by which, when it endeavours to recover its equilibrium, it produces electric motions. Not that I think we may preſume to diſcuſs the nature of the firſt elements of the electric fire, and of its primitive forces; all theſe are beyond the reach of our ſenſes, conſequently beyond the ſphere of our underſtanding. We are to look for the principles on which we mean to ground our theory, in the laſt phenomena to which our ſenſitive faculty can extend.

913. Now the laſt phenomena to which my ſight, my feeling, and in general my ſenſes have allowed me to reach, have all of them confirmed this truth to me. I. That, *the electric fire, neither in inſulating nor in deferent bodies, can either be dilated by drawing it to contrary parts, or condenſated by trying to compreſs it from oppoſed parts.* This firſt truth, as far as concerns glaſs, is conformable to the Franklinian theory, and is demonſtrated in a very ſimple manner by the experiment related at p. 64 of the *vindicating* electricity; which conſiſts in touching both coatings of a plate, with the

the hooks and the coatings of two bottles equally and ſimilarly electrified; when the coatings of the plate are found to have contracted no electricity. With regard to ſuch other ſolid bodies, as may be electrified in the ſame manner as glaſs, the like reſult will be obtained *. With reſpect both to inſulating and deferent bodies of a fluid nature, the ſame is manifeſt from the experiment of the electric *well*; in fact, the natural ſtate either of the fire of the cavity of the *well*, or of the bucket ſuſpended in its middle, or of the air contained in it, are not in the leaſt altered, whatever electricity may be introduced into the ſubſtance of the well.

914. *The electric fire can be condenſated or dilated on alternate ſurfaces, or ſtrata, of inſulating bodies, provided at the ſame time it is condenſated or dilated on the other alternate ſurfaces.* This ſecond truth, is the ſame as the Franklinian principle concerning charges of ſheets of glaſſes, or inſulating ſtrata of any kind. Fire can be condenſated on the internal ſurfaces of a ſeries of bottles, A B, C D, when the hooks of every one communicate with the coating of that which ſtands next; ſo that the fire may be dilated at the ſame time on all their external ſurfaces. The ſame we have ſeen to take place, with regard to ſtrata of inſulating ſubſtances: for inſtance, from the interior ſurface of the *well*, a certain quantity of fire may be driven away, and condenſated in the air contiguous to it, provided an adequate portion may be likewiſe driven from that ſurface of air, which is contiguous to what we called the *ſoul* of the *well*.

915. III. *Within the ſubſtance of deferent bodies, the electric fire cannot be either dilated, or condenſated, in any permanent manner.* This third truth is manifeſt from the experiment formerly related,

* We are not to object againſt the principles laid down above, that when a glaſs plate is rubbed on both its ſurfaces jointly, there reſults on both theſe ſurfaces, homologous electricities, and that the ſame takes place on a ſilk ribbon. The act of rubbing, either leſſens or increaſes the capacity of the rubbed body, as I have ſhewn at the end of the chap. III. It is not therefore ſurpriſing, that both ſurfaces in ſuch caſe, ſhould either give, or receive fire; but as ſoon as both recover their natural capacity, they immediately endeavour either to throw out fire, or draw the ſame, till they have recovered their former natural quantity: from all this reſults the intenſe atmoſpheres that take place around inſulating bodies, when rubbed.

as

of the electric *well:* the bucket when extracted from the bottom of it, brought no electricity along with it; now, if any electricity had been condensated within this bottom, the bucket would have manifested some sign of it; but to say it once more, electric fire can never be condensated without a correspondent dilatation of the same. Hence, the inalterability of the natural quantity of electric fire in deferent bodies, which at first appeared to us to be a kind of inexplicable mystery, is found to be a consequence of an universal propriety of this fire, viz. of refusing, in any medium or body whatever, to be either compressed or dilated; unless it be in certain particular cases, when alternate condensations and dilatations of it take place in the same directions: which alterations cannot take place in deferent bodies, in consequence of their very deferency *.

916. IV. Over the surface of deferent bodies, electric fire may be permanently condensated or dilated; but this is only when a *correspondent quality of natural fire may alternately be condensated or dilated through successive strata of the air, contiguous to them.* This proposition constitutes the principles on which the theory of electric atmospheres, explained in Section III. is grounded, and is a consequence of the experiments on the electric well, which I have just quoted. On the surface of the body E (Pl. VIII. fig. I) fire may be permanently condensated on the concavity of the successive strata of contiguous air, correspondently to the fire that may be dilated on the convexity of the same, &c.

* I confine this proposition to the case of *permanent* condensations, arising from the contrary actions of simple atmospheres; though, in the case of a spark that runs into the substance of a deferent body, or out of it, there is also a kind of transient condensation of the fire; which is manifest from the effects produced by such sparks: but then these condensations proceed from forces that all conspire to produce them in a certain sole direction; besides, these condensations do not take place within the deferent substance, otherwise than correspondently to the alternate condensations and dilatations which they may create in the natural fire of the ambient air, against both the air that surrounds the wire, and that which is contiguous to the ground: whence it happens, that those who are distant even fifty paces from the place which is struck, feel a great shock, especially in their legs; this I propose to treat more minutely in another book in which I shall deduce from observation, the more exact and precise precautions that are to be used for conducting lightning.

917.

917. V. *The natural fire of the air which is situated in the middle between two bodies similarly electrified, cannot be alternately condensated or dilated, but in oblique directions outwards, as I have explained when treating of the electricity which is exerted from the upper cavity of the well.*

918. VI. *It therefore follows, that neither the excess nor deficiency of two bodies electrified alike, can be accumulated on the internal surfaces * of these bodies, but are compelled to flow to the external surfaces of the same.* If two bodies, E and *e* (Pl. VIII. fig. 5.) are electrified by excess, this excess will endeavour, from opposite parts, to compress the natural fire of the air lying between them; which will prevent an alternate succession of dilatations and condensations from taking place: now, as fire cannot be condensated on the surface of bodies, but as far as there is a possibility for the above alternate succession of dilatations and condensations, the consequence will be that on the *internal* surfaces of bodies, no fire can be accumulated, but will be compelled to condensate itself on their external surfaces, that is, on those places where the condensating forces do not counteract each other. It may be proved after the same manner, that on the *internal* surfaces of bodies no dilatation of the natural fire can take place, and can only arise on their external surfaces.

919. VII. *The excess and the deficiency of two bodies contrarily electrified, must be accumulated on their internal surfaces.* Because there the excess in E, and the deficiency in D (Pl. VIII. fig. 7) endeavour, in the same direction, to produce alternate condensations and dilatations of the fire in the air between these bodies; that is, the excess in E, as it accumulates itself against the air internally contiguous to it, endeavours to drive the natural fire from the air contiguous to D; and reciprocally, the dilatation of the fire in the air internally contiguous to D, endeavours to draw the accumulating fire from the body E, against the air internally contiguous to the same: now, as no such co-operating actions as these take place externally, both the excess in E, and the deficiency in D, will be accumulated

* By the word *internal surfaces,* the author means those which face each other.

on their internal ſurfaces. To expreſs the ſame effects otherwiſe, I might ſay, the exceſs in E is a force that internally drives the fire remaining in D; and the deficiency in D is like a ſupport that is taken from the exceſs in E.

920. From all the propoſitions above, the following ſimple principle is to be deduced: *all motions of electric bodies are effected from the places of the greateſt reactions of their atmoſpheres, to thoſe where ſuch reactions are leaſt, and ſuch motions are beſides proportioned to the differences of theſe reactions*; or in other words, *all electric motions ariſe from places in which the alternate condenſations and dilatations of the electric fire cannot be effected, or are effected with a great difficulty, to thoſe where they can be effected with greater eaſe.*

921. With regard to bodies poſitively electrified, the caſe is manifeſt; they reciprocally recede from each other: that is, they move from the middle or internal ſpace between them, in which the alternate condenſations and dilatations on the one ſurface, cannot correſpond to thoſe that ariſe on the other, and where conſequently the exceſſive fire is reciprocally repelled from theſe internal ſurfaces, and move towards oppoſite external parts of the ambient air, where they can eaſily affect alternate dilatations and condenſations, and where exceſſes of fire can be accumulated againſt the contiguous air: that is, from the place where the reaction is greateſt, they move towards the place where it is leaſt.

922. The ſame is alſo manifeſt with regard to bodies negatively electrified. From the middle interval, in which alternate condenſations and dilatations cannot ariſe in conſequence of the dilatation that takes place on both the internal ſurfaces of ſuch bodies, and where therefore the remaining natural fire is reciprocally attracted to theſe internal ſurfaces, the bodies move towards oppoſite external parts of the air, where they can effect alternate dilatations and condenſations, and where conſequently deficiencies of fire can be accumulated, correſpondently to the fire that may be relaxed towards the external ſurfaces of the contiguous air.

923. With regard to bodies contrarily electrified, that which is poſitively electrified moves towards that which is negatively electrified, where a deficiency favours thoſe alternate condenſations

and

and dilatations which the former endeavours to introduce into the natural fire of the contiguous air: and reciprocally, the body negatively electrified, moves towards that which is positively electrified, where an excess favours the alternate dilatations and condensations which the former endeavours to introduce into the natural fire of the contiguous air.

924. And thus, since the production of alternate condensations and dilatations, always depends on the state of the two bodies, it follows, that *approaches* and *recessions* of electrified bodies are always reciprocal.

925. In the fourth place, if a body E (Pl. VIII. fig. 3.) be positively electrified, and the body N be not electrified at all, the alternate condensations and dilatations will be equally on all sides around the body E; because the excessive tension which will be introduced by the excess in E into the air externally contiguous to the body N, will be equivalent to the excessive tension which the excess in E introduces elsewhere into the air internally contiguous to the same body N; which according to the hypothesis, retains its whole natural quantity of fire, and no more; therefore this body N will be balanced between excessive tensions that will be equal on all sides. The body E will be likewise balanced between excessive equal tensions, viz. between the tension which the excess of E introduces into the air in the middle, and the reaction of the equal tension which the whole fire of N will introduce into the fire of the external air †.

926. In the same manner, two bodies, the one D negatively electrified, and the other N, not electrified, (Pl. VIII. fig. 4.) will both remain balanced in the fire of the air around them, which is every where equally relaxed: the reason is, because the relaxa-

† Universally, the reaction which arises from the fire repelled to the external surface of a body N is less, according as this surface is ampler, and spreads a tension into ampler strata of air: under such limitation we are to understand this paragraph and the next. The more the external surface of N will surpass the internal surface of it, the less the reaction against the external surface will be; and the more imperfectly the equilibrium will subsist.

tion of the fire in the external air, relatively to N, will be equivalent to the relaxation which, from the deficiency of D, will arife, in fome other place, in the fire of the air which is immediately and fucceffively contiguous to the body D; and the relaxation of the fire of the fame external air, (fince N is fuppofed to retain its natural quantity of fire) will be equivalent to the relaxation of the internal air.

927. Here we have therefore, if I do not miftake, brought electric motions to an *unity* together, and explained the efficient caufe of them, without having recourfe to two diftinct currents of electric fire; a fuppofition not only fuperfluous, but totally repugnant to the evidence of facts.

928. The caufe above affigned to electrical motions, befides its conformity to phenomena, has this farther advantage to recommend it, that it unites and proves to be the fame with the efficient caufe of fparks. The reafon why the fire which is redundant in a fyftem leaps, condenfated into one fpark, towards another fyftem which is relatively deficient, is nothing more than the excefs of reaction which the fire of the ambient air exercifes in fome place againft the fire of that fyftem, above the reaction exercifed againft the fame fire, by the fire of the air between the two fyftems. And indeed it is very natural, that as electric figns are fubject to the fame laws, they fhould arife from the fame caufes.

929. Is it not befides this fame reaction which, by being in fome particular place increafed, drives the fpark into a given direction, that is the caufe why, in the inftant this fpark takes place, that it is in the inftant of an actual diffufion of the fire, the velocity is increafed with which bodies contrarily electrified move towards each other. In fact, if the mere relaxation of the natural fire in the air between two bodies, produces their mutual approaches of *fimple preffion,* thefe fame motions muft be more quickly performed as the relaxation increafes in confequence of the bodies being brought nearer to each other: this motion muft likewife be more rapid when the relaxation of the natural fire in the

the air between the two bodies, is turned into an actual motion, when the remaining refiftance actually yields to a reaction which is fomewhere elfe fuperior, and when the electric fire relatively redundant around a fyftem, drives from the fubftance of it an equal portion of fire into the fubftance of another fyftem which is relatively deficient. Thefe pofitions are entirely confentaneous to the univerfal principle, viz. *that any folid body, or fluid, whatever, preffes againft adjacent fubftances with a part of the force by which it is actuated, that is lefs in proportion as it employs a greater fhare of this force in its own actual motion* *.

930. If the principles above are properly applied, they may alfo with the greateft clearnefs explain other more complicated electric motions. Certainly, we may difpenfe with faying any thing about electric ofcillations; fince they are formed by the mutual approaches of bodies contrarily electrified, and the mutual receffion of the fame, when become fimilarly electrified by virtue of their mutual contacts; only, fuch vibrating motions ufually take place in that body alone which is placed in the middle, in confequence of the immobility of thofe placed at the extremities.

931. The above ofcillations are turned into mere *fufpenfions*, when the body finds itfelf in the middle between equal active parts of the fame current. The little pendulum fufpended in dilated air, as we faw before, remains fufpended in the middle of the rare continued ray, and does not vibrate through fhort ofcillations but in the inftant when the current by which this ray is formed, begins or ends, and confequently prevails on either fide of the pendulum. In the fame manner, a pendulum of a confiderably greater weight than the former, remains fufpended in the middle of the fulgid, continued, and loud ftream of fire, which fometimes runs between the two bells with which I explore the ftate of the atmofpheric electricity: it feems that, as

* The words of the original are the following,—*Che un corpo o fluido qualunque, o folido, con tanto minore porzione della forza qualunque, da che fi animato, preme le aggiacenti foftanze, quanto è maggiore la porzione di quella forza, che impiega nell' attuale movimento fuo.*

the

the former thin ray fuppreffes the reaction of the rare remnant of air in the bell, fo the denfe ftream of fire in the latter cafe, alfo fuppreffes the ftrong reaction exercifed againft the heavy pendulum by the denfe atmofpheric air.

932. The manner after which a gold leaf remains fufpended, feems to confirm the above explanation: it feems that the diftenfion of it muft be imputed to an excefs of the reaction exercifed on its plain furfaces, above that exercifed on its fides and angles, which find themfelves in the middle of a continued ftream. Is not the reaction of the electric wind, which in thofe fhort inftants when it prevails over the electric reactions which decreafe in confequence of the fire that actually diffufes itfelf; is not, I fay, this reaction the caufe why the gold leaf thus circumftanced flips away?

933. But in the motions of a *difpofition or direction of bodies into the fire's way*, I think I ftill better difcover the reality of the caufe which I am affigning here. When I fee a fufpended drop of water, lengthen itfelf towards the water that is prefented underneath it in a deferent veffel, and mean while fee the latter rife towards the drop under the fhape of a little hill, I cannot help thinking but that the fire which from the lower part of the drop diffufes itfelf into the water and thence into the ground, does not exercife the fame degree of action and reaction on both this drop and the water, which the natural fire in the adjacent air exercifes in the fpace around that fame drop, and upon the even furface of the water, around the little hill raifed above it; becaufe, in the fame manner as the natural fire in the air contiguous to a body, fuffices to drive the natural fire out of that part of it which is immerfed, for inftance, in the atmofphere of the Machine, fo I think that the natural fire of the air contiguous to the metallic leaf, fuffices to render its furfaces even, and diftended, in confequence of the lefs reaction exercifed on its points, through which the fire actually flows. And thus alfo I think that the natural fire of the air contiguous to the water, fuffices to raife it towards that place where the fire actually flows, and of courfe exercifes a lefs reaction.

934.

934. Again, when I fee particles of bran rife from a metallic bafon, and unite together into rows or ftrings, and the latter remain fufpended between the conductor and the bafon, in order to conduct the fubtle continued ftream of fire, I think that this obtains, becaufe the particles of bran, in the place of their mutual contact, where the fire freely flows, meet with a lefs reaction than in any other place externally, where the natural fire being affixed to the air, exercifes a confiderable reaction ; or even in any other place internally, where, for inftance on the bafon or the conductor, the fire is more diffufed and divided.

935. The transformation of mutual approaches into mutual receffions, confirms this idea to me. Since this transformation takes place when the electric fire, which, in order freely to pafs from a body into another makes both move to each other, is at laft retarded, and obliged to accumulate itfelf fo as to have its reaction much increafed, it is natural that when the ftream of fire is increafed beyond the quantity which the particles of bran are capable of conducting, the ftrings made of fuch particles fhould break, and thefe particles feparate.

936. Conformably to the fame principle, mutual approaches are likewife transformed into mutual receffions, when the traverfing electric fire excites either in the bodies, or in the medium around them, fuch alteration as may caufe the action arifing from it, to furpafs the decreafing reaction which refults from the paffage of the fire: as for inftance, when fpark a drives moifture into vapours intenfely repulfive, or deftroys that part of the body out of which it rufhes, or into which it runs, and fometimes drives it into vapour: in both thefe cafes it explodes the bodies towards oppofite parts.

937. And laftly, the experiment of the *aereo vaporous* electricity, which has been lately defcribed, alfo confirms the theory of the reaction of electric atmofpheres, and their efficiency in producing electric motions. In fact, all the experiments that I have related with regard to vaporous air pofitively electrified, equally fucceed when air is negatively electrified. And as the natural fire of bodies is balanced by the natural fire in the air,

fo

ſo the natural fire, whether increaſed or diminiſhed in vaporous air, is balanced by the natural fire of thoſe ſame bodies, which is likewiſe encreaſed or diminiſhed. As the exceſſive fire of bodies is accumulated on their ſurfaces, and creates a particular tenſion in the natural fire of the air over it, when unmixed, ſo the natural fire of bodies accumulates itſelf on their ſurfaces, againſt the diminiſhed and relaxed fire of vaporous air. As the natural fire of unmixed air exerts itſelf againſt the fire that is relaxed on the ſurface of bodies, and drives it away, ſo the fire increaſed in vaporous air drives the natural fire from the ſurface of bodies, &c. Here follow a few differences between the above two caſes. I. The unbalancement between the fire of bodies, and that of unmixed air, ariſes only from an alteration in the quantity of the natural fire of theſe bodies; but the unbalancement between the natural fire of bodies, and the fire of vaporous air, may ariſe from an unequality in the alteration of the quantity of fire in the vaporous air, and in thoſe bodies. II. The latter unbalancement is of leſs duration than the former, in proportion as the unbalanced fire may more eaſily diffuſe itſelf through the vaporous air, than through unmixed air.

938. If I could flatter myſelf that I have ſatisfactorily explained all the above phenomena, I would venture to expreſs my ſatisfaction, by concluding this chapter with the experiment of an electric dance, which occurred to me a few years ago. At all events, I will ſubjoin the experiment here; I ſuſpend two long very fine flaxen threads, to the lower ſurface of the conductor: I wet them and keep them ſeparated from each other, till the electricity is grown ſomewhat intenſe; then leaving both at liberty they begin to dance, inceſſantly vibrating their extremities, and continue twiſting and untwiſting each other, in contrary directions, alternately: that is to ſay, from the interval between them, they throw their extremities outwards, and throw a moiſt effluvium equally electrified with themſelves, into the ambient ſpace; and as their electricity is thus diffuſed within a certain plane, they are repelled in ſome other plane which has received no ſuch electricity.

Second

Second part of the Sixth Section.——On the electric motions of insulating bodies, and on the *vindicating* electricity.

CHAP. I.

On the electric motions, and the vindicating *electricity of insulating bodies of a rare texture, consequently incapable of a charge.*

939. THE laws concerning the electric motions of deferent bodies, are also applicable to insulating bodies. I. Two insulating bodies similarly electrified, move from each other. II. Two insulating bodies contrarily electrified move towards each other. III. Between an insulating body that is electrified, and another which is not, no motion takes place.

940. The same law takes place likewise between a deferent, and an insulating body. I. An insulating body, and a deferent body, similarly electrified, move from each other. II. An insulating body, and a deferent body, contrarily electrified, move to each other. III. And if of two bodies, the one insulating, the other deferent, the one be electrified, the other not, and no diffusion of electricity can take place between them, no motion will arise between them.

941. But here we must observe two very remarkable differences. I. Two insulating bodies, or an insulating and a deferent body, contrarily electrified, after joining together remain strongly united; in which they differ from deferent bodies, which having in their mutual contact equalized their respective quantities of fire, repel each other in proportion to the fire that remains in both; or part without showing any farther electric sign, if their excess and deficiency, being at first equal, have reciprocally annihilated each other. II. After an insulating body has joined with another insu-

lating body, or with a deferent body, and they have loſt their contrary electricities (ſo far as theſe were equal) the inſulating body, as ſoon as it is taken from the other, *recovers* its former electricity: to this property I have reduced the many various phenomena which have been obſerved, partly by others, and partly by me, with experiments on ſeveral kinds of inſulating bodies. It is this property which I have expreſſed by the words, *vindicating* electricity.

942. I think I have been the firſt (as Dr. Prieſtly obſerved in p. 271 of his hiſtory) who gave inſtances of the motions of *inſulating* bodies, of their mutual adheſion, and of their *vindicating* electricity. The inſtance which I produced in the 196 p. of *Natur. Elec.* I had been ſupplied with by Signor Alexandro Vaudania, whom I had deſired accurately to obſerve the electric phenomena or accidents, which happened to him when he undreſſed himſelf; and who, in conſequence, ſent to me the following account. "Since the cold is grown ſharper, that is, for theſe ten or twelve days paſt, I have put on, between my two linen ſhirts, a ſhirt or under-garment of beaver. Every night, when I take off my upper ſhirt, which I change every day, I perceive that it has a kind of adheſion with the beaver garment; and when I ſeparate them I hear a number of cracks, produced by ſparks; theſe, when obſerved in the dark, do not differ from thoſe which take place in electric experiments. When I afterwards take off the beaver under garment, I feel it ſticks ſtill more to my under ſhirt: after forcibly ſeparating them, I perceive that the lower edge of the beaver continues to adhere to the ſhirt, and draws it off from my body: having entirely ſeparated it from the ſhirt, the latter flies to my body; I then approach again the beaver, and the ſhirt flies to join it again; I ſeparate them, and the ſhirt flies to my body." In theſe facts we manifeſtly ſee the inſulating beaver adhere to the linen ſhirt and loſe its electricity, and then, by virtue of a forcible ſeparation from it, recover it; whence it is afreſh enabled to join with the ſhirt, to draw it off, &c.

943. Signor Symmer afterwards promoted much the ſame kind of experiments, being led into it by the ſparks which he heard and ſaw, when drawing his ſilk ſtockings at night. After many

many trials he found that the electric phenomena became particularly intenſe when he put on a white ſilk ſtocking, and upon this another of black ſilk: he obſerved that as long as theſe were upon his leg, they gave, though rubbed ever ſo ſtrongly, but very weak ſigns of electricity: when they were drawn off together, they hardly gave any; but when he afterwards drew the white ſtocking from the black one, moſt vivid ſigns of electricity aroſe, by exceſs in the white ſtocking, by deficiency in the black one. Being kept at ſome conſiderable diſtance from each other, they remained both ſwelled as if his leg had been ſtill within them: bringing them nearer to each other, the ſwelling was gradually diminiſhed, their mutual attraction encreaſed, and they at laſt precipitated themſelves towards each other with an amazing violence, and cloſely united: being again disjoined, they exhibited the ſame ſigns of electricity, the ſame phenomena. When the ſtockings were kept ſeparated, they quickly loſt their electricity, as does a rubbed tube; but when they were left joined, they preſerved it for two hours, or even more. Nor was it poſſible, when they were in ſuch a ſtate to give them, or to draw from them, any electric fire, not even with the ſharpeſt point: their union at the ſame time was ſo ſtrong, that a weight of ſeventeen ounces ſuſpended to one of them could not ſeparate them: nay, when the black ſtocking was freſh from the dying, and the white one recently whitened with ſmoke of brimſtone, they reſiſted to a weight of three pounds three ounces, &c. When, to two ſtockings thus ſticking to each other, the ſame gentleman preſented two others more ſtrongly electrified, viz. a black one to the white one, and a white one to the black one, the former immediately parted and united with the latter. When the ſtockings thus preſented were electrified in a leſs degree than the former, the former remained united, their adheſion was only weakened by the preſence of the latter, and they all weakly united together into one maſs. The ſame Signor Symmer alſo obſerved, that not only electrified ſtockings united together, but alſo remained ſuſpended to deferent bodies.

944. The Abbé Nollet in the third volume of his Letters, has made ſeveral obſervations on the experiments of Signor Symmer; he

he rubbed ſtockings ſtretched on a deferent plane, and afterwards transferred the experiments from ſtockings to ribbons, which he electrified after ſeveral different manners. Signor Cigna, phyſician, in the third volume of the Royal Society of Turin, has carried farther the experiments of Signor Symmer, and of the Abbé Nollet, with ſilk ribbons, of which he combined the number and colour after different manners: he beſides rubbed them on planes inſulated, deferent, ſmooth, rough, hairy, &c.

945. In order to treat this very complicated matter with as much clearneſs as poſſible, I think we muſt begin with obſerving what kind of electricity is introduced into a given inſulating body of a rare texture, in conſequence of a friction made againſt it, when ſuſpended in the air, by another given body. With regard to this I have obſerved, I. That by keeping a ribbon ſtretched in the air, and rubbing a ſide of it with a given body, there ariſes in it a weaker electricity, than if both its ſides were rubbed at once. II. When the ribbon thus ſuſpended was rubbed on both its ſides at once with different bodies, the one of which was apt to create an electricity by exceſs, the other an electricity by deficiency, there reſulted in it an electricity proportioned to the difference between the intenſities of the contrary electricities that would have reſulted from ſeparately rubbing the two ſides of the ribbon, firſt with the one of the two above bodies, and then with the other.

946. I have afterwards found that if an inſulating body A, of a rare texture, be ſtretched on a plain body B, and be rubbed upon it with another body C, the electricity that riſes in A, is proportioned to the ſum of the ſimilar electricities, or to the difference of the contrary electricities, which are introduced by the friction of C, and the *counterfriction* of B. That is to ſay, I ſuppoſe that the body A which is rubbed, has its parts ſo flexible that the body C, by moving along the one ſurface of the ſaid body, and all the way preſſing upon it, communicates a nearly equal motion to the other ſurface of it which is ſtretched on the plane B: from this preſſing progreſſion of A on B, ariſes what I called the *counterfriction* of B againſt A; this moveableneſs or flexibility, I have found to take place, not only in ſtockings, but alſo in ſeveral compact kinds of ſilk ſtuffs, for inſtance in ſattin. Therefore, if with

with ivory, with iron, with braſs, with copper, I rub a white ribbon on a plane made of any one of thoſe ſame bodies, as they all draw fire from the ribbon, the latter will become negatively electrified. Likewiſe, if I ſtretch the ſame white ribbon on a board of walnut-tree, of beech-tree, or on a black ſilk ſtocking, and rub it with a piece of any one of theſe bodies, as they all give fire to the black ribbon when ſuſpended in the air, the latter will become poſitively electrified. If with one of ſuch bodies as give fire, I rubb the ribbon on one of ſuch bodies as receive it, then no electricity takes place, or at leaſt a very weak one, the nature of which is very often uncertain ; a circumſtance which in ſeveral experiments has perplexed me : but at laſt this electricity prevails which is moſt active ; and this activity may be either increaſed or diminiſhed by ſeveral accidents. Here follows an inſtance of it : a little blade of gold always electrifies by exceſs a white ribbon ſtretched in air, either when the friction is made with the plain ſurface of the blade, or when it is made with the angular ſide of it *. Likewiſe, if I ſtretch a white ribbon on a plate of glaſs, and rub it there with the plain ſurface of the blade, ſo that thence ariſes no counterfriction from the glaſs, or at leaſt a very ſmall one, the ribbon alſo acquires a ſmall electricity by exceſs : but if I rub the ribbon with the angular ſide of the blade, ſo that by inſerting itſelf, as it were, into the furrows of the ribbon, it thus produces a ſtrong counterfriction from the glaſs, there ariſes in the ribbon a negative electricity, proportioned to the exceſs of force which the glaſs has of drawing off the fire of the ribbon, above the force which the gold has of ſupplying the ſame.

947. To the preceding rule which is laid down with regard to a body rubbed when ſtretched on a plane, this other is entirely conformable, which has for its object the electricity that reſults within two flexible ſubſtances, placed together upon a plane. If,

* I have repreſented in the fig. 10. of the Pl. X. the inſtruments which I uſe for rubbing ribbons ; they conſiſt of a wooden handle, and of a blade of iron or of ſteel, when I mean to rub with iron, or with ſteel : but this ſame blade is lined underneath with a thin plate of gold, or of ſilver, &c. when I propoſe to rub with either of theſe metals.

for

for inftance, two white ribbons are placed on each other, then ftretched on an infulating plane, and the upper one is rubbed with any body whatever, it will be found that when the two ribbons are, after the friction, taken from the plane, either jointly, or the one after the other, they will repel each other, and manifeft homogeneous electricities, which will be either pofitive or negative, according as both the rubbing body, and the contra-rubbing plane are apt to give, or to draw off, fire; or they attract each other, and manifeft contrary electricities, if the rubbing body is apt to give, and the *counter-rubbing* plan to *draw off, fire, and vice verfa :* that is to fay, the upper ribbon in all cafes manifefts the fame electricity which it would manifeft, if, being alone and ftretched in the air, it were rubbed by the above body; and the under ribbon manifefts the fame electricity it would, if, being alone and ftretched in the air, it were rubbed by the *counter-rubbing* plane. Thus, if I ftretch two white ribbons on a plate of glafs, and rub them there with ivory, with iron, with copper, with brafs, &c. as the latter bodies receive in this cafe, fire from the ribbon which they rub, and the counter-rubbing glafs alfo draws fire from the other ribbon, both will become negatively electric; but if I ftretch the two ribbons on a plate of fealing wax, or fulphur, and there rub them with fealing wax, or with fulphur, or with a black ftocking, they will both become pofitively electric. Laftly, if the ribbons being ftretched upon any one of the latter bodies, which are apt to give fire, and rubbed with any one of the former, which are apt to draw fire, or (*vice verfa*) the ribbons will manifeft contrary electricities.

948. V. If the plane on which the fimilar ribbons are rubbed, be deferent, or though being of an infulating nature, does not actually completely infulate, either becaufe it is not polifhed enough, or becaufe the ribbons themfelves are imperfectly dry, then the law laid down in the preceding paragraph, is proportionably difturbed. The reafon of this will foon be underftood if we confider that any electricity endeavours to turn an homologous one into a contrary one, if the latter be lefs intenfe, and is enabled to diffufe itfelf away: it therefore may eafily happen that the electricity of the upper ribbon will turn that in the under ribbon, into a

contrary one, whenever the fire in this under-ribbon will be able freely to diffufe itfelf through the plane. To this cafe may be reduced all the alterations that may take place in the preceding rule.

949. If two flexible bodies poffefs different electric properties: if the one, for inftance, be a white, and the other a black ribbon, then the electricities that will refult in them, will be conformable to the electricity which the rubbing body will introduce into the upper ribbon, and to the electricity which the plane will introduce into the under-ribbon; and befides, to the alterations that fhall arife in thefe electricities, from the reciprocal friction between the two internal furfaces of the ribbons.

950. By means of the precedent rules, I flatter myfelf that I have clearly expreffed the laws that determine the nature of the electricities that refult from frictions; now I fhall endeavour to defcribe the manner after which thefe electricities take place. Firft, I fhall propofe two cafes, fimple and luminous, which fhew, how immediately after a friction that has been performed in the air, electricities refults in two ribbons, the one white, the other black. I fhall afterwards avail myfelf of thefe examples in order to examine the electricity of other bodies: ribbons rubbed in the air, are electrified in a fhort fpace of time, and retain for a long while the electricity that is communicated to them.

951. I therefore hold with one of my hands, the extremity of a ribbon of fine white filk, three lines broad, and a foot, or a foot and an half long. I embrace both its furfaces with a piece of black velvet, very dry, or even warm, (warmth difpofes bodies more plentifully to give their electric fire), and preffing the ribbon within the velvet, I make the latter move along the former; and after a fhort while the ribbon grows ftrongly electrified in a pofitive manner: another perfon mean while does the fame with a black ribbon warmed, and a piece of white velvet, and the black ribbon becomes ftrongly negatively electrified.

952. Let us now proceed to examine the manner after which the electricity exerts or difplays itfelf from rubbed ribbons, immediately after their friction: I think that it is the fame as that in which it exerts itfelf from the plate of cryftal formerly mentioned, (Pl. I. fig. 10.) I think that the *rubbing black velvet increafes the capacity*

pacity of the white ribbon, and thus depofits in it fuch quantity of fire, as the friction may render it capable of receiving : but when afterwards the friction ceafes on a given part of this white ribbon, I think that it recovers its lefs capacity, and that as the fire depofited by the black velvet then becomes *exceffive*, it conftitutes an abfolute excefs on the furfaces of the ribbon ; whence refults its intenfe atmofphere. I likewife think, that in the act of the friction performed by the white velvet on the black ribbon, the latter diminifhes the natural capacity of the former, and the black ribbon in confequence lofes its fire ; but as that part of it which gets out of the friction, immediately recovers its natural greater capacity, there refults a deficiency on both its furfaces ; and thence arifes its intenfe negative *electricity*. Of this my idea I have, befides its analogy with other well afcertained facts, a proof from immediate experience. Another perfon and I hold a ribbon ; I rubb the one furface of it with any body whatever ; the other perfon prefents a flaxen thread to the other furface ; and we find that it never moves towards the place that correfponds to the friction, it only runs, even obliquely, to the place where, on the other furface, the friction has ceafed. This evinces, that in the ribbon as well as in the cryftal plate, the electricity only begins to manifeft itfelf, when the rubbed furface gets out of the friction.

953. And now, thefe confiderations naturally lead us to propofe the following queftions. I. When I rub a ribbon on a plane, and when after the friction, the former adheres to the latter, does it in fuch ftate retain its electricity, or does it lofe it, and only retain a difpofition to recover it, when feparated ? II. When the white and the black ribbon, by virtue of the contrary electricities which they have contracted, run to each other, or when either of them runs to join with the table, with the wall, &c. do they, in this ftate of adhefion, retain their actual electricities ; or can we fay that they lofe them, and only preferve a difpofition refpectively to recover them afterwards, in the act of their mutual feparation ?

954. The opinion has, it feems, generally prevailed, that infulating bodies, when electrified and brought to the ftate of adhefion juft mentioned, preferve their actual electricities ; and it

it was the more natural for every body to acquieſce to this opinion, as this kind of adheſion does only take place between two inſulating bodies, or between an inſulating body and a deferent one, when ſuch inſulating bodies, or body, are actually electrified, and as the continuation of this adheſion ſeems to evince a continuation of the electricities; beſides, the nature of the electricity that again takes place after the ſeparation, abſtracted from other conſiderations, ſeems to prove the continuation of it, in the ſtate of adheſion.

955. Yet, in the firſt place, I obſerve that in the inſtant when I ſeparate, in the dark, a ribbon from the table on which I rubbed it, there appears a track of light on the ſucceſſive places where the ſeparation is made; a proof this, that the ſucceſſive parts of the ribbon, as they are ſeparating, begin to give ſigns of electricity, and to be in a different ſtate from thoſe parts which remain joined to the table, which give no ſign at all. I therefore conſider this track of light as a token, both that the ribbon had, through the friction, depoſited its fire into the table, and that it recovers it, when it ſeparates from it.

956. In the ſecond place I alſo obſerve, that I have no ſooner rubbed a white ribbon with black velvet, than it runs, if left at liberty, to my ſleeves, to my coat, and ſticks to them: nor is this all, but ſome parts of the ſaid ribbon apply themſelves and ſtick to ſuch parts of the ſame, as already adhere to my body. Now, how is it poſſible to reconcile this union of the two parts of a ribbon, if both retained an homologous electricity, with the law, that bodies ſimilarly electrified mutually repel each other.

957. In the third place, the phenomenon is ſtill more inſtructive, of the reciprocal adheſion of a white ribbon with a black one, both contrarily electrified: this adheſion is inverſely proportional to that electricity which remains in both ribbons, when thus united; this remaining electricity is of the ſame kind with that which, before the union of the two ribbons, predominated in either of them; and again the adheſion is inverſely proportioned to this primitive exceſs. Let the white ribbon be more ſtrongly electrified by exceſs,

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than the black one is by deficiency; the ribbons in this cafe will indeed run to each other, but then mutually adhere with lefs force than if the deficiency in the black ribbon had been equal to the excefs in the white one; a redundant electricity will prevail in the two united ribbons; and if the point of a needle be moved along the black ribbon, the two ribbons will after this ceafe to give any farther figns of electricity, the overplus of the electricity diffufing itfelf into this needle; and the adhefion of the ribbons will moreover encreafe.

958. Laftly, the fine experiment which Signor Cigna, a Phyfician, relates in the third chapter of his differtation, entirely convinces me, that in the act of uniting an infulating body with a deferent one, or alfo an infulating body with one contrarywife electrified, both electricities are annihilated, and that when the two bodies are feparated, the electricities are produced afrefh. The faid gentleman held a ribbon pofitively electrified by one of its extremities, and prefented it to the fmooth plain furface of a plate of lead infulated with filk threads, in a vertical plane: and he firft obferved that the ribbon was but flowly drawn by the plate; which circumftance is conformable to my principle, that an electrified body does not join with a body that is not electrified, but after it has introduced into it an electricity contrary to its own: now, as the ribbon was pofitively electrified, it could not drive away the natural fire of the plate by means of its own atmofphere, but fo far as the latter was imperfectly infulated, and in proportion to the intenfity of the atmofphere which it was able to actuate on the oppofite furface of the fame. But if at the fame time, that the above Gentleman prefented the ribbon to the plate, he alfo prefented his finger to the latter, a fpark, he fays, leaped from the lead to his finger; the ribbon, at the fame inftant flew to the plate and remained adherent to it; nor did either of them give after this any electric fign. Now, was not this fpark correfpondent and equal to the excefs which the ribbon depofited into the lead? Was it not therefore a fign that the ribbon, in uniting to the lead, gave up its own actual excefs? Laftly, Signor Cigna feparated the ribbon from the lead, when the ribbon

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appeared electrified again; and then the fame Gentleman, with his finger, returned a fpark to the lead: that is, the ribbon, when taken from the plate, carried off part of the natural fire of it, leaving it in a deficient ftate, and thus recovered its own former excefs.

959. Here we muft obferve that the above experiment is equally conclufive in any fuppofition whatever; becaufe we muft in all cafes admit the two following propofitions, viz. that two bodies fimilarly electrified repel each other, and that two bodies contrarily electrified attract each other. The fpark which leaps from the lead to the finger, when the ribbon joins with the former, is able to electrify a third body in fuch a manner as to caufe it to be repelled by this ribbon, when the latter is afterwards feparated from the lead; this fpark therefore is the electricity itfelf which belonged to the ribbon. On the other hand, the fpark which leaps from the finger to the lead, when the ribbon has been feparated from it, leaves fuch an electricity in that finger, as to caufe it to be attracted by the ribbon, when it is thus feparated from the lead; fo that this electricity is a contrary electricity to that in the ribbon. Whence, in any fuppofed cafe, we may fay that the ribbon in uniting with the lead diffufes its own electricity through it into the finger; in parting from the lead, it refumes this electricity; hence it happens that the finger afterwards finds the lead in a negative ftate.

960. From the above propofitions and facts it manifeftly follows. I. That electrified infulating bodies lofe their actual electricity when they enter into a ftate of adhefion. II. That in the act of their feparation, they recover it. Which principles being once laid down, cannot it alfo be faid that this difpofition which is left in the infulating body by its late electricity, of afterwards recovering it, is alfo the caufe why this infulating body, while thus deprived of its electricity, remains in the above ftate of adhefion. But of this more accurate proofs will be given in the following chapter.

CHAP. II.

On the vindicating electricity of compact solid insulating strata.

961. THE first phenomena that have been observed with regard to the *vindicating* electricity of compact insulating strata, were those, a notice of which was sent by the Father Jesuits at Pekin, to the Academy of St. Petersbourg, in the year 1755, and which may be read in the VIIth volume of the new commentaries of this Academy. Signor Symmer in his third Memorial, which was read in the Royal Society of London, the 20th of December 1759, says he charged two thin sheets of glass, joined together by their naked surfaces, and externally coated; when the charge was completed, he took the upper plate, by two of its angles, and when he raised it, he saw that the under plate stuck to it, and remained suspended to it; when he had discharged the plates, the adhesion ceased. He recharged the two plates, then having inverted them when thus united, he made the plate that communicated at first with the Chain, communicate now with the ground, and that which communicated with the ground, communicate with the Chain; when he found that after the electrization had, in this state of things, been continued a certain time, all adhesion ceased. Using afterwards two plates coated on both their contiguous surfaces, he found that no adhesion took place. Signor Symmer makes use of these two experiments in order to confute the theory advanced by certain philosophers, of two electric fluids, the one *affluent*, the other *effluent*; he pretends that each of the two distinct united glasses may be considered as the one of the surfaces of a single plate; that one of the glasses is impregnated with an electricity of one kind, and the other glass with an electricity of another kind; he moreover is of opinion that the adhesion of the two naked plates

plates of glafs is a demonftrating proof of the exiftence of two antagonift forces.

962. Signor Cigna, in the fourth chapter of his Differtation, carried ftill farther the experiment of the Fathers of Pekin, and of Signor Symmer. He relates that two naked glaffes, by rubbing the upper furface of them, remained united, both to each other, and to the gilt paper, or the fheet of lead, on which they were placed; that in this ftate they gave no fign of electricity; that if they were then feparated from the paper, or the lead, they manifefted on their two external furfaces the fame kinds of electricity; that if the paper or lead was again joined to the glaffes, the electric figns again ceafed; that if the paper, or lead, was kept parted from the glaffes by means of a filk ribbon, the paper or lead manifefted an electricity contrary to that of the glaffes; that if the glaffes were likewife kept feparated from each other, they alfo manifeft contrary electricities.

963. I do not propofe to repeat all the numerous experiments which I related in my book intitled, *Obfervationes atque experimenta quibus electricitas vindex latè conftituitur & explicatur.* I am actually employed in promoting my enquiries on this fubject, and if I meet with fome fuccefs, I propofe to publifh what difcoveries I fhall be able to make. Mean while I fhall only repeat in this place, the experiment which is made with the two plates, A B, *a b*, M N, *m n*, (Pl. XI. fig. 4.) jointly charged, and I fhall exprefs the fucceffive effects of the *vindicating* electricity in this experiment with the figure 2.

964. And firft, in order to perceive the unity which really takes place in all the phænomena of the *vindicating* electricity, however contrary to each other fome of them may appear, it muft be obferved, I. That the law of the vindicating electricity of compact infulating ftrata, for inftance, plates of cryftal, is the fame with the law of the *vindicating* electricity of rare infulating bodies, for inftance, filk ribbons. II. That the whole fpecific difference between them lies in the former being capable of a charge, which the latter are not. III. Thence it refults that the alterations of electricities, which are readily affected with bodies of a rare texture, by dif-

joining,

joining and rejoining them, are not ſo with compact inſulating ſtrata; ſuch alterations are confined to thoſe ſurfaces of the latter which are kept joined together by the contrary electricities of the other two ſurfaces, which conſtantly endeavour to preſerve *their contrariety to each other, and their equality with the electricity of the ſurfaces which are united together.*

965. For inſtance, I. Two ribbons contrarily electrified, when they unite together, reciprocally deſtroy their electricities, and thus remain adherent. After the ſame manner, if two plates A B *a b*, M N *m n*, are joined by their reſpective ſurfaces, *a b*, M N, contrarily electrified (I ſuppoſe the ſurface A B to be poſitively electrified, and the oppoſite *a b*, negatively; therefore M N is poſitively electrified, *m n* negatively) theſe two contrary electricities will endeavour to deſtroy each other; the redundant fire in M N will endeavour to diffuſe itſelf into *a b*, and fill up its deficiency; but this reciprocal ſuppreſſion of electricities cannot be effected otherwiſe than by a joint annihilation of the exceſs in A B, and of the deficiency in *m n*; therefore, in conſequence of the impenetrability of the plates, ſome external communication becomes neceſſary; and this will no ſooner be procured, than the exceſs of M N will diffuſe itſelf into *a b*, when the electricity of the two ſurfaces *a b*, M N, will be annihilated after the ſame manner as the electricities of the two ribbons were before.

966. Again, the two ribbons, when they are ſeparating, freely recover their electricity, which they had readily loſt when they joined; and in the ſame manner, the two plates M N *m n*, A B *a b*, in the inſtant they are ſeparating, endeavour to recover on their ſurfaces *a b*, M N, the electricity they have loſt in conſequence of their union together, and of the communication of their external ſurfaces. Yet it is to be obſerved that the ſurface M N, in its endeavour to recover its exceſs, is reſtrained by the difficulty which the inſulated oppoſed ſurface *m n* experiences in diſmiſſing an adequate part of its own fire; and the ſurface *a b* likewiſe, in its endeavour to recover its deficiency, is reſtrained by the difficulty which the oppoſite ſurface A B experiences in recovering an adequate exceſs; whence it happens that the two disjoined plates,

plates, I. Manifeſt electricities reciprocally contrary. II. Similar electricities take place over the two oppoſite ſurfaces of the ſame plate. III. And this electricity is of the ſame kind as that recovered by the disjoined ſurface.

967. The reaſon is, that in disjoining the two ſurfaces *a b* M N, I. The ſurface M N, by endeavouring to recover its former exceſs, endeavours at the ſame time to drive away a quantity of natural fire from the oppoſed ſurface *m n*. Now, as the latter remains inſulated, it cannot transfuſe any fire into the ground, neither can it accumulate any within its coating *c d* ; it therefore muſt accumulate it on the open ſurface of this coating, againſt the contiguous air : ſo that there will reſult an exceſſive tenſion in the natural fire of the ambient air, and a redundant atmoſphere around *m n*. II. Likewiſe, in the act of the ſame ſeparation, the ſurface *a b*, in endeavouring to reſume its former deficiency, draws, according to the Franklinian theory, a certain quantity of redundant fire, to the oppoſite ſurface A B : now, as this ſurface remains inſulated, it cannot derive this fire from the ground, neither can it draw it from the internal ſubſtance of its own coating; it muſt then draw it from the outer ſurface of this coating, that is, from the ſurface of the contiguous air (if before ſeparating the plates, the coatings are taken off, the experiment will equally ſucceed.) Therefore, a particular relaxation will ariſe in the natural fire of the air around the plate A B ; there will reſult a deficient atmoſphere.

968. This explanation how the atmoſpheres ariſe, which take place over the ſurfaces oppoſite to thoſe which are disjoining, likewiſe ſuffices to explain the ſingular circumſtance of ſimilar electricities ariſing over oppoſite ſurfaces of the ſame plates. If while the two plates A B *a b*, M N *m n* are ſeparating, two ſharp points are kept preſented to their external ſurfaces, the bruſh appears on the point directed to A B, and the ſtar on the other which is directed to *m n* : the ſame force which, when the points are preſenting, draws a bruſh to A B, and drives the fire that forms another bruſh from *m n*, this ſame cauſe I ſay, when theſe two ſurfaces remain inſulated, draws to A B the natural fire of the contiguous air, creating a deficient atmoſphere over it, and throws

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exceffive fire from *m n* into the air contiguous to it, raifing in it a redundant atmofphere.

969. That afterwards, over the external furfaces correfpondent to *a b*, M N, when they are feparating, atmofpheres arife that are homologous to the electricity which thefe furfaces recover, is what appears natural, when we confider, *that the latter furfaces refume, by virtue of their feparation, greater electricities than thofe which can poffibly be raifed on the oppofite furfaces, which are infulated.* This principle being admitted, it follows that if the furface M N, cannot drive from the oppofite furface *m n*, a quantity of fire fufficient to produce in it a deficiency equal to the excefs recovered by the fame M N, it follows, I fay, that a portion of this excefs muft flow outward, againft the contiguous air, and there produce a redundant atmofphere. Likewife, if the furface A B cannot draw to itfelf a quantity of fire fufficient to produce in it an excefs equal to the deficiency recovered by *a b*, it follows that this A B muft, from the air contiguous to it, draw a certain quantity of fire, and thus produce a deficient atmofphere over itfelf. That is to fay, the excefs redundant in, and flowing out of, M N, againft the air contiguous to it, *ipfo facto* leffens the excefs in this M N, and thus brings it to a ftate of lefs unequality with refpect to the deficiency actuated in *m n*; and the fire which from the contiguous air flows into A B, *ipfo facto* leffens the deficiency in it, and thus brings it to a ftate of lefs unequality with regard to the excefs in *a b*.

970. Thefe explanations of the *vindicating* electricities of two plates, may be demonftrated by the experiment in which, after jointly charging and difcharging them, I continue for an hour and more to obtain fparks by touching them when feparated, and again touching them when rejoined; and reciprocally, the above explanations throw a complete light on that fame experiment, which I never could repeat without exciting the wonder of thofe who were unacquainted with electrical operations, and attracting the attention of the Philofophers who came to fee my experiments. I join the two plates A B *a b*, M N *m n* together, by their naked

furfaces

furfaces in contact with each other; and then introduce into the coating CD, for inftance, the electricity of the Chain; the charge being completed, I difcharge them; this done, I feparate them, and touch the coatings; I join them again, and then again touch them; and thus doing, I continue to excite a very long feries of fparks: here follows the manner after which I operate.

971. I begin with exciting fparks from the coating alone of the upper plate; that is to fay, I. I continually touch with one of my fingers the under coating *c d*. II. When I feparate the plate A B, I take care not to touch its coating CD. III. Having feparated this plate, I immediately touch it and give a fpark to it; that is to fay, I give to A B an excefs adequate to the deficiency contracted by *a b*, at the inftant of the feparation. IV. I ceafe touching A B; I rejoin the two plates, and touch again CD, and draw fparks from it, by means of which I draw off the excefs I communicated to A B after the laft feparation, and which it does no longer require, when in a ftate of conjunction. V. Proceeding thus with the ufual caution not to touch the coatings in the act of feparating, or of rejoining the plates, I continue to give fparks after every feparation, and take them back after rejoining the plates.

972. In general, the fpark which I draw after rejoining the plates, is more divided than that which I gave after feparating them. In very favourable weather, after feparating the plates, I often draw two or more fucceffive fparks; but after rejoining them, the fire that leaps from my finger is completely united into one fpark, and much more vivid.

973. In order to underftand the reafon of this difference, we muft confider, I. That the fire which flies from *a b*, in confequence of the deficiency which now takes place in it, goes to M N in order to form the excefs which this M N wants; therefore, as an excefs arifes in A B, in confequence of my touching it at times, fo a deficiency arifes in *m n*, in confequence of its conftant communication with my hand. II. When I rejoin the two plates, the excefs I have introduced into A B cannot be annihilated but fo far as the excefs in M N runs to fill the deficiency in *a b*; and the excefs in M N does not depart, but when I give fire to *m n*, in order to

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fill its deficiency. III. In fact, if, while I rejoin the plates, I keep my finger at a diſtance from *m n* (or its coating *c d*) then I cannot draw from A B the exceſs I introduced into it; becauſe as I do not then fill the deficiency in *m n*, the exceſs cannot be annihilated in M N, nor the deficiency in *a b* ſupplied. IV. However, when I touch *m n* (or *c d*) while I rejoin the plates, the exceſs of A B is not for all that thrown out at once, becauſe the ſurfaces *a b* M N, do not inſtantaneouſly touch each other in all their parts; hence a ſlowneſs and ſucceſſiveneſs take place in the reſpective anhilations of the exceſs in M N, of the deficiency in *a b*, and of the exceſs in A B. V. But when after ſeparating the plates I preſent my finger to C D, (or A B) the exceſs is at once thrown to it from my finger, owing to the violence with which the whole A B then wants an exceſs adequate to the deficiency then completely formed in *a b*.

974. Conformably to what has been ſaid above, we muſt take care that every time that the plates are joined, they be preſſed together for ſome few ſeconds of time, in order that the ſmall charges which have been formed by the ſeparation, may have time both to diſſipate entirely, and to ariſe again with more ſtrength, when the ſeparation will be again effected.

975. I. If after touching the plates when rejoined, they are again disjoined without drawing a ſpark, and then rejoined, no ſpark will be thrown from A B, becauſe it has, in ſuch caſe, received no fire. II. If after touching the plates when ſeparated, they are rejoined, then disjoined again, without previouſly drawing a ſpark, A B then receives no ſpark, becauſe it has given none at the time of its laſt joining with the other plate; *ſo true it is that inſulating bodies contrarily electrified, are diſpoſed, when they join together, mutually to annihilate their reciprocal electricities, as well as to recover them again, when they are ſeparated.*

976. I have hitherto, in the experiment of the two plates, only examined that kind of electricity which is common both to compact inſulating bodies, and to thoſe of a rarer texture: I mean that kind of electricity, *by virtue of which they recover, when ſeparated, the electricity which they had loſt by their being joined*

together,

together, and which I call positive vindicating electricity. Now, I shall in the same experiment, examine that kind of *vindicating* electricity which is proper to compact insulating bodies, *and by virtue of which, when they are separating from one another, they give up the electricity with which they had been impregnated*; this I call *negative vindicating electricity.*

977. Having therefore jointly charged the two plates A B *a b*, M N *m n*, I begin the operation of successively disjoining and rejoining them: in order to effect this more easily, I clip one of the angles of the one of the plates; and then I observe, I. That the plates, when they are disjoining, manifest signs of a *negative vindicating* electricity. II. They afterwards reach to the last limit of this electricity. III. Then successively follow, for a very long space of time, to give signs of a *positive vindicating* electricity. That is to say, I. At first, the surfaces *a b*, M N, when they are separating, lose a part of the electricity with which they are impregnated. II. Then they reach a certain term at which they do not, notwithstanding they are again separated, lose any more of the electricity which remains in them, nor recover any portion of that which they gave up when the *negative* vindicating electricity began to act, or even afterwards when the *positive vindicating* electricity began to take place.

978. In the meanwhile, the similarity of the atmospheres that take place over the two surfaces of the same plate, both when the *positive vindicating* electricity, and the negative one obtain, though it has been looked upon as fatal to the Franklinian theory, really proceeds from the following principle, which is the foundation of this theory, which is, *that the contrary electricities of plates, which by virtue of the separation of the latter, are become unequal on each opposite surface, severally endeavour to return to a state of equality*; *that is to say, that electricity on the one of the two surfaces, which the separation has caused to have grown less, endeavours to lessen the electricity on the other surface; and vice versa, that electricity which, in consequence of the separation, is become superior to its opposite one, tends to encrease the latter.*

976. Therefore, when I at first begin to separate the two plates A B *a b*, M N *m n*, the excefs of M N and the deficiency of *a b* endeavour mutually to leffen each other; but the other two furfaces A B, *m n*, being infulated, their refpective excefs and deficiency are not altered; that is to fay, the exceffive fire is, as it were, drawn from M N into *a b*; the deficiency in *a b*, thus becomes lefs than the excefs in A B, and endeavours to leffen it; it therefore drives a portion of this excefs in A B, againft the air contiguous to it, and thus creates the redundant atmofphere over A B: and reciprocally, the excefs in A B being now greater than the deficiency in *a b*, endeavours to encreafe it; it drives a part of the fire remaining in this *a b*, into the air contiguous to it, and raifes over it a redundant atmofphere. Likewife, the excefs in M N being become lefs than the deficiency in *m n*, endeavours to leffen it, it draws fire into *m n* from the air contiguous to it, and thus renders its atmofphere ftill more deficient; and reciprocally, the deficiency in *m n*, being greater than the excefs in M N, endeavours to draw fire into the latter, from the air contiguous to it, and thus raifes a deficient atmofphere over it.

980. On the other hand, when after the rife of the pofitive *vindicating* electricity, I again feparate the plates, both the excefs in M N, and the deficiency in *a b*, continue to be reproduced, though the contrary correfpondent electricities cannot arife on the furfaces A B, *m n*, which remain infulated: therefore the greater deficiency in *a b*, endeavours to increafe the leffer excefs in A B, by drawing the natural fire from the contiguous air into it, and thus raifes over A B a deficient atmofphere; and reciprocally, the lefs excefs in A B endeavours to leffen the deficiency in *a b*; to that end it draws fire into it from the air contiguous to it, and thus raifes over it a deficient atmofphere. Likewife, the greater excefs in M N endeavours to encreafe the deficiency in *m n*, driving its fire from it into the air contiguous to it, whence refults a redundant atmofphere over *m n*; and reciprocally, the lefs deficiency in *m n* endeavours to leffen the excefs

excefs in MN; to that end it drives a part of the latter's redundant fire into the air contiguous to it, and thus raifes a redundant atmofphere over it.

981. Conformably to thefe principles. I. When I feparate the plates AB *a b*, MN *m n*, for the firft time after their being charged, they refift fo much the feparation, that there is great danger of breaking them. II. From the coating CD a ftrong fpark leaps to the neareft finger of that of my hands which holds the plate AB *a b*, and the edge of its coating CD appears all round fparkling with very vivid brufhes: all this demonftrates to me that a diminution of the excefs of AB takes place, at the inftant when the deficiency of *a b* is forcibly leffened. III. Likewife, in the act of the fame feparation, a ftrong fpark flies from the finger with which I hold the plate MN *m n*, to its coating *c d*, and its edge appears all round fhining with vivid fparks; this manifefts to me that a diminution of the deficiency of *m n*, is effected at the fame time that the excefs of MN is forcibly leffened. IV. Meanwhile, the flafhes of light which appear between the furfaces *a b*, MN, while they are feparating, are produced by the fire which, by virtue both of the excefs in AB which remains fuperior to the deficiency in *a b*, and of the deficiency in *m n*, which remains fuperior to the excefs in MN, endeavours to leap from the above *a b* into MN. V. In this ftate of things, the upper plate AB *a b* repels a white ribbon from both its furfaces; over which, as has been explained in the preceding paragraph, fimilar redundant electricities take place. VI. On the contrary, the under plate, MN *m n*, repels a black ribbon from both its furfaces, by virtue of the deficient atmofphere, which as hath been alfo explained, takes place over both its furfaces.

982. The plates being joined again, the intenfity of thefe attractions and repulfions leffen; becaufe the excefs of MN, and the deficiency of *a b* are now refpectively kept back by the external deficiency of *m n*, and the external redundancy of AB. The adhefion of the plates takes place again, but in a lefs degree than formerly, proportionably to the diminution which the original charge has fuffered from the firft feparation; and by pro-

ceeding to a fecond feparation, the fame phenomena continue to take place by virtue of the fame caufes as formerly, though their intenfity is proportionably leffened.

983. Continuing thus to join and feparate the plates, we pretty foon attain a term at which, I. The plates ceafe to manifeft any fenfible adhefion. II. In feparating them no light appears. III. After the feparation, they do not fenfibly draw or attract rubbed ribbons. This term is the point of the contrary inflexion, the limit between the negative *vindicating* electricity which takes place at firft, and the pofitive one which fucceeds to it. This term is fooner attained, according as the infulation of the plates is lefs complete: in this cafe one plate fometimes reaches to this term a little before the other, which ftill continues to draw and repel ribbons with a fenfible degree of force. Laftly, this term is attained, before the effect of the feparations has entirely annihilated the charge introduced at firft into the plates. In fact, if they are rejoined immediately after the term is paffed, they ftill give pretty ftrong fhocks.

984. If, after the term is paffed, the plates are fucceffively joined and feparated, but without touching them; they begin, by virtue of thefe fucceffive feparations, to recover their former electricities; that is, the furface *a b* of the plate A B *a b*, begins to recover a part of what deficiency it had at firft, and the furface M N, begins to recover alfo a part of what excefs it may have loft. Whence it happens that, after the feparation, the deficiency of *a b*, being become greater, endeavours to encreafe the excefs of A B, by drawing into it the natural fire of the air contiguous to it; and reciprocally, the excefs of A B, being lefs than the deficiency of *a b* endeavours to leffen it, by drawing into the fame *a b*, the natural fire of the air contiguous to it; fo that *ab* and A B then begin to repel the black ribbon. Likewife the excefs of M N, being become greater than the deficiency in *m n*, endeavours to increafe it, by driving the fire of *m n* into the air contiguous to it; and reciprocally, the deficiency of *m n*, being lefs than the excefs of M N, endeavours to diminifh it, by driving the fire of M N into the contiguous air, whence M N and *m n* begin to repel the

the white ribbon. And thus the *negative vindicating* electricity becomes changed into a *positive vindicating electricity*.

985. By continuing thus to rejoin and disjoin the plates, those portions of electricity that had been lost are pretty quickly recovered on all sides, by virtue of these successive separations; the adhesion of the plates, and the repulsion of the ribbons also encrease in proportion; so that it appears that all these phenomena of the *positive vindicating electricity*, continue till that degree is attained, at which the charges that had been introduced at first, are annihilated.

986. Beyond this term, if the plates are continued to be rejoined and disjoined, for an whole hour or more, without being touched, they continue to shew some adhesion to each other; they continue when separated, to repel ribbons conformably to the kind of electricity which they have resumed on their internal surfaces, &c.

987. I have represented in the fig. 2. of the Pl. XI. the series of the above alterations of the vindicating electricity. Now I shall make use of a figure of the same kind, in order to explain the vindicating electricity of the plate M N *m n*, (Pl. XI. fig. 4.) The same explanation will serve for the electricity of its fellow-plate; only, the ordinates must be taken on the other side of the abscis. Let the two equal right lines O F, *o* F represent the excess introduced into M N by the charge, and the deficiency introduced into *m n*. On the first separation of the plates, M N will, for instance, lose the portion *u* F of its excess: therefore, it will in consequence of this separation appear negatively electrified over both its surfaces; the plates being joined again, it will recover part of its former excess, and will thus be brought to have then the whole of its excess equal to P G. In consequence of a new separation, a portion *x* G of the same excess will again be lost; and thus it will at last happen, that M N will have that precise degree of excess at which a further separation can no longer lessen it; so that H is the point at which the *vindicating* electricity begins to be altered, that is, from negative becomes positive. At a following separation, by virtue of which the remaining excess is already reduced to the less value R I, the plate, instead of continuing

tinuing to lofe any more of its excefs, on the contrary begins to recover the portion of it I y. Hence, as the remaining excefs from the charge, in M N, is gradually reduced to the lefs values K S in K, L A in L, and o in M, the furface M N gradually recovers gradually greater portions of its former excefs, K s, L z, M &. From that point afterwards the furface M N, by virtue of other fucceffive feparations, will for a very long while continue to recover portions of its former excefs, which (the operation being continued without touching the plates) will gradually vanifh at every fucceffive conjunction of the fame.

988. And thus the portions of a curve O Q M, oq M, will, with their refpective ordinates, exprefs the exceffes and deficiencies, both primitive and remaining, of M N and $m\,n$; the portions of a curve u H & v, V H & V, will, with their ordinates, exprefs as far as H, the negative *vindicating* electricities, and beyond H, the *pofitive vindicating* electricities, of the furfaces M N, $m\,n$. The fame portions of curve which ferve to exprefs the degrees of pofitive and negative vindicating electricities that take place at every fucceffive feparation of the plates, will alfo ferve to reprefent the progreffion of the mutual adhefion of the plates. u F, U F will exprefs the greateft degree of the adhefion of the plates, when they ftill retain their whole charge; which value will gradually leffen conformably to the fucceffively leffening ordinates, x G, X G; at the inftant when the negative electricity will take place, this value will be o in H, that is, at the point of the contrary inflexion; and thence it will continue quickly encreafing, then very flowly decreafing, conformably to the fucceffive ordinates. I y, I Y, K s, K S, L z, L Z, M &, M &, &c.

989. With refpect to the experiments that are made on the *vindicating* electricity of a fingle plate A B $a\,b$ (Pl. I. fig. 1.) by disjoining its coating C D, they differ much in point of intenfity and duration, from the experiments that are made with the two plates jointly charged. Of this difference the caufe partly at leaft is manifeft: in the feparation of the two plates jointly charged, the *vindicating* electricities of the two furfaces which are disjoining, co-operate together; and this circumftance muft in-

increafe the effects, and better preferve the efficient caufes; that is, the difpofitions introduced by the charge of the plates, by virtue of which they endeavour to difmifs their refpective electricities to a certain degree, and beyond this degree, to recover the fame.

990. With regard to the manner after which the fame vindicating electricities exert themfelves, I obferve, I. That pofitive *vindicating* electricities exert themfelves after the fame manner, when only one plate is ufed, and feparated from its coating, as when both are ufed, and fucceffively feparated from each other. II. Negative vindicating electricities alfo exert themfelves after the fame manner, if the charge introduced into the fingle plate is very weak, confifting for inftance, of only two or three fparks from the firft conductor; becaufe the charge which is ufually introduced into the joined plates, is likewife fmall, on account of the thicknefs of the whole. III. But if the charge introduced in the fingle plate be much intenfe, then the phenomena which refult from disjoining the coating of it, while the plate retains its whole charge, are proportionably different from the phenomena which refult from feparating the two plates, when they only poffefs their *joint* charge.

991. That is to fay, each of the plates that retain their charge, manifefts in confequence of a feparation, the fame electricity on both its furfaces, with that of the furface which is disjoined; but the plate which has been charged alone and poffeffes a confiderable degree of charge, manifefts that kind of electricity on the furface which is disjoined from its coatings, which is proper to that furface; and the contrary kind of electricity on the other furface. Thus, if the fingle plate A B *a b* be ftrongly charged, pofitively in A B, negatively in *a b*, it will, after the coating C D is taken off, repel a white ribbon from A B, and a black ribbon from *a b*.

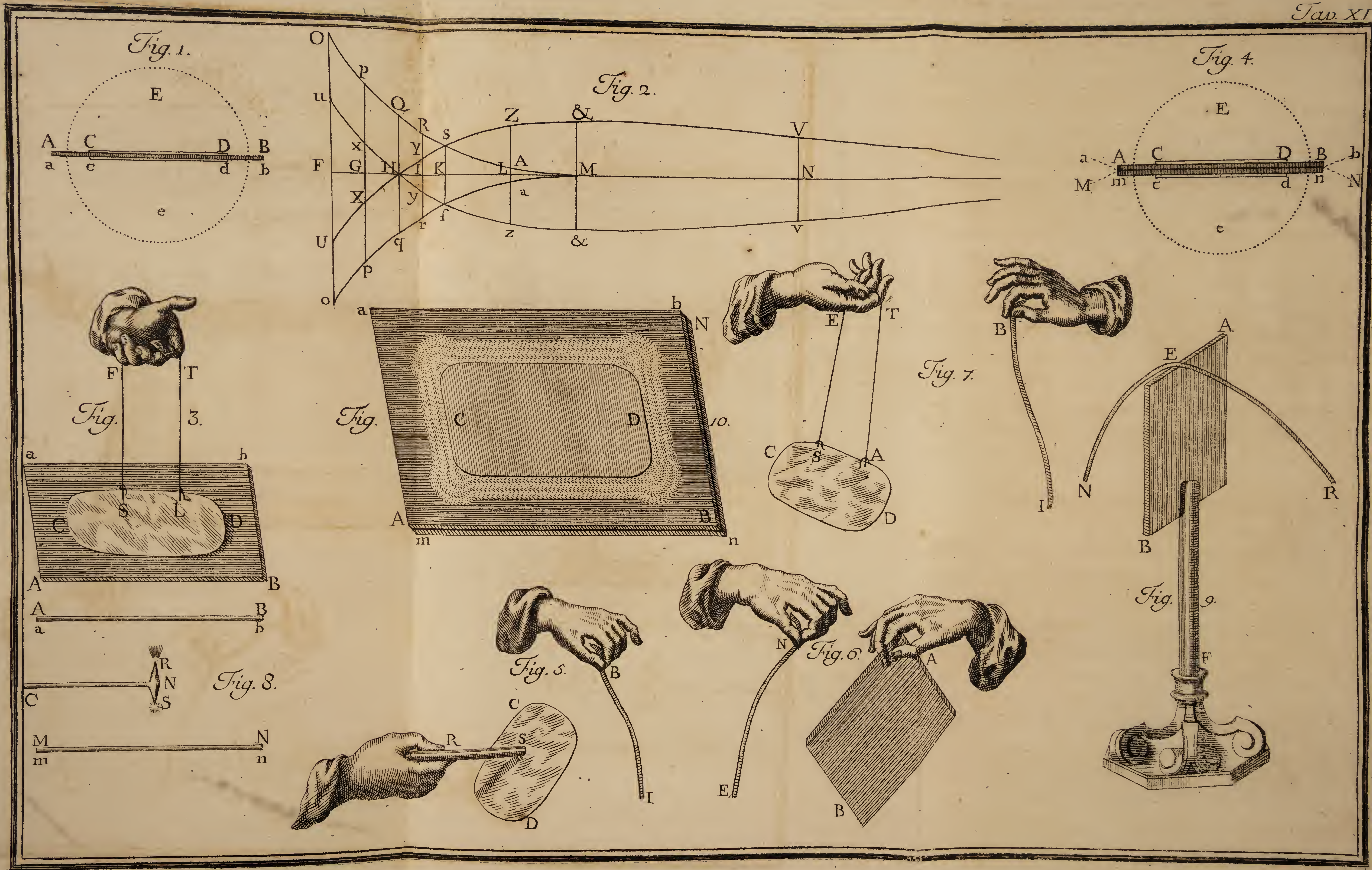
992. The reafon of this is, that charges univerfally endeavour, with a force proportioned to their intenfity, to grow gradually lefs; and this force counteracts the force with which they endeavour

to keep their ſtate of mutual equality, the force by which the ſingle charged plate endeavours, when ſeparated from its coating, to *actuate ſimilar atmoſpheres in the air contiguous to its two ſurfaces.* When I take off the coating C D from A B *a b*, which I ſuppoſe to be ſtrongly charged, I leſſen the electricity of A B; therefore, by virtue of the force with which the two contrary electricities conſtantly endeavour to keep their ſtate of equality, the deficiency in *a b* muſt leſſen, and the exceſs in A B of courſe ſomewhat increaſe: as the electricity on both ſurfaces ſtrongly endeavours at the ſame time to grow leſs in conſequence of its very intenſity, the deficiency in *a b* very ſtrongly leſſens by the united efficiency of the two above cauſes, and the exceſs of A B, even after the ſeparation of its coating, will continue to decreaſe a little, in conſequence of the leſſening force, which ariſes from the intenſity of its charge, and ſurpaſſes that which tends to an equality; thence, a certain quantity of fire flows from A B into the contiguous air; but *a b* at the ſame time draws fire from the air contiguous to it with very great force, and after this manner the above effects take place.

993. I have repeated the above obſervations from my above mentioned book on the *vindicating electricity*, and added ſome new ones, in order to throw ſome more light on the ſubject: with regard to the nature of the adheſion which accompanies *vindicating* electricities, I ſhall only ſubjoin two trials I have made about it. The firſt is as follows; if two plates, either charged, or lately diſcharged, and which therefore ſtrongly adhere to each other, are immerſed into an extenſive ſubtle flame, or, when taken from this flame, are ſuſpended within a large glaſs bell, emptied of air (Pl. X. fig. 11.) they ſoon part from each other. The other experiment is that of disjoining bodies naturally joined, for inſtance, ſtrata or ſheets of talc, or of *ſpato:* no electricity at all ariſes from theſe bare ſeparations. With reſpect to the cauſe of the *vindicating* electricity, and of the adheſion that accompanies it, it certainly would, if diſcovered, throw a conſiderable light on the properties of inſulating bodies, on the manner of their

charges,

Tav. XI



charges, on the nature of electric atmoſpheres, and conſequently on all the moſt ſtriking phenomena of electricity, ſuch as the *bruſh*, the *ſtar*, and the electrical motions. A conſideration this, which is very apt both to excite us to inveſtigate ſuch cauſe, and reſtrain us from barely *imagining* it.

F I N I S.

ADVERTISEMENT.

Though the following treatiſe on the ſtate of Atmoſpheric Electricity, during ſerene weather, does not properly belong to the ſubject of the Work juſt now concluded, yet, as it is a late publication of the ſame Author, and its ſubject is both new and intereſting, it has been thought it would not be diſagreable to the reader to find it added to this book. The numbers of the paragraphs have, in conſequence, been continued from the preceding ones, as the Author himſelf has done, for the greater convenience of references and quotations.

ON TERRESTRIAL ATMOSPHERIC ELECTRICITY, DURING SERENE WEATHER.

Of its perpetuity and conſtant manner of acting, and of the alterations that take place in its intenſity, in conſequence of the changes in the ſtate of the air.

LETTER I.

To his Exc. D. Angelo Maria Carrone di S. Tommaſo, Marquis of Aigueblanche, Firſt Secretary of State for Foreign Affairs, &c.

994. AS in the diſcharge of the high Office to which your hereditary loyalty, moſt excellent Sir, and your talents have raiſed you, you conſtantly endeavour to ſecond the views of our gracious Sovereign, that is to ſay, to ſecure and promote the public happineſs,—ſo your laudable taſte for all that relates to natural hiſtory, as well as the deep queſtions you have of late propoſed to me, beſides the friendſhip with which you have conſtantly honoured me, induce me to hope that you will condeſcend to caſt your penetrating eye on the following propoſitions, in which I treat of that perpetual motion which, in the atmoſphere that ſurrounds us, obtains in that moſt active element, which is both the principal motor and performer of the greater part of natural meteores. If the propoſitions in queſtion meet with ſuch a favourable reception from you, which advantage I flatter myſelf they will obtain, their ſubject being both new and intereſting, it will be a great encouragement to them to proceed farther, and make their appearance before the Royal Prince, to whom, amidſt the univerſal and joyful congratulations of the people,

people*, it is likewiſe fit that Natural Philoſophy ſhould offer its applauſe.

995. Firſt propoſition. I. *How to ſettle a proper apparatus for exploring the electricity of the atmoſphere, during ſerene weather.*

996. Ever ſince the year 1757, I have written at length, (and I think conformably to truth, as far as I can hitherto perceive) concerning the different properties of conductors, for indicating the ſtate of the electricity of the atmoſphere in general; which properties vary according to the ſubſtance, the length, bigneſs, ſhape, and height above ground, of theſe conductors. With reſpect however to the electricity of the atmoſphere, during ſerene weather, it is to be inveſtigated only by uſing *exploring* wires of metal, that are very long, and eſpecially placed very high; for as ſuch electricity is commonly very weak, it would, by operating otherwiſe, often prove a difficult matter to aſcertain its exiſtence, and much more ſo its nature.

997. For that ſame reaſon, viz. the weakneſs of the electricity to be examined, I do not recommend ſharp points; they would too readily draw off and diſſipate ſuch electricity.

998. Theſe previous eſſential cautions being laid down, I think I cannot better continue to indicate the manner in which a pretty good apparatus may be ſettled, than by ſhortly deſcribing that with which I am obſerving this very inſtant.

999. In the firſt place, with regard both to the *openneſs* and height of its ſituation, it is ſettled on the pleaſant hill of *Garzegna*, which is ſituated in the neighbourhood of Mondovi; and from which the whole compaſs of the Alps, as well as the whole plain of Piedmont, is eaſily diſcovered. This vaſt extent both of country and ſky, that opens before the above hill, I take particular notice of, as contributing much to the ſucceſs and certainty of the experiments.

1000. The ſubſtance of the apparatus conſiſts of an iron wire, an hundred and thirty two French feet long, this I call the *exploring* wire. It extends from a ſtack of chimnies over which

* The Author means to ſpeak of the circumſtance of the marriage of the Prince of Piedmont, with a Princeſs of France, which was then on the point of being celebrated.

it

it is raifed by a long pole, to the top of a cherry-tree to which it is faftened; its extremities are infulated, and over each a fharp fmall umbrella of tin is placed, the under part of which is coated with fealing wax. Laftly, another iron wire is faftened to the middle of the former; and being thence continued, and let into the room in which I obferve, through a piece of cryftal fixed in a wooden pane in the window, and fecured in this piece of cryftal with fealing wax, it brings me conftant information of the ftate of the electricity in the *exploring* wire: I therefore call it, the *deferent* wire.

1001. This apparatus, which though it is much lefs extenfive than thofe which I have hitherto ufed, either in this place, or in others, yet I find fufficient to difcover the different motions of atmofpheric electricity, during ferene weather.

1002. Propofition II. *The electricity during ferene weather, in its middle and ordinary ftate, makes two little balls of pith of elder, one line in diameter, diverge from a fmall fheet of metal placed between them, fo far as fix lines each: thefe balls are fufpended by very fine filk threads, fixteen lines long.* II. *In the ftate of its greateft intenfity, this electricity makes the above balls diverge fo far as fifteen, twenty, or more degrees from the fmall fheet of metal.* III. *In its weakeft ftate, it is only to be difcovered by the mutual approach and junction of thefe balls, from very fhort diftances.*

1003. I conftantly meafure the intenfity of fuch electricity from the divergence it produces, and not from the fparks that are thrown off, becaufe in its weakeft ftate, it never rifes fo high as to afford any fpark that are in any degree difcernible; and even in its middle ftate, thofe it affords are only perceivable by a very dry fenfible hand, or by prefenting the tip of one's nofe to the wire, in the meanwhile taking care not to breathe. When the electricity increafes beyond this middling ftate, then indeed the fparks it throws are more fenfible, but yet the difference between its fucceffive degrees cannot be fo well afcertained as by means of the fimple divergence of the above balls; efpecially becaufe an electrometer with fparks, would obftruct the continuance of the obfervations, as will appear from the following propofition.

1004.

1004. Propofition III. *The atmofpheric electricity during ferene weather is fometimes fo flow, as that, being deftroyed by the wires being touched, it takes one minute or more before it becomes again fenfible; and at other times, it is fo quick as to become again fenfible after a fecond's time.*

1005. In expreffing the above degrees, either of the intenfity or *frequency* of the electricity in queftion, I mean to fpeak of fuch obfervations only as I have made with the above mentioned apparatus; for in proportion as the extenfivenefs, and height above ground, of the apparatus, will be increafed, the intenfity and frequency of the electricity, every other circumftance being equal, will alfo increafe.

1006. Propofition IV. *The atmofpheric electricity during ferene weather, is always of the exceffive, otherwife, pofitive kind. In the very rare inftances that happen of this electricity being deficient or negative, it is then only brought over by the wind, from fome part of the fky, though perhaps very diftant from the place of the obfervation, which is either foggy, or fnowy, or rainy, or cloudy.*

1007. Ever fince I began to obferve the atmofpheric electricity during ferene weather, the whole feries of my obfervations has confirmed it to me, that this electricity is conftantly of the exceffive, or pofitive kind.

1008. On the mountain of St. Michael, I indeed happened three times in fifteen days, to find, during ferene weather, the wires I had fettled there in March 1767, (See Let. p. 134) electrified by *deficiency*. But then I took notice that the high mountains over the town of Sufa, from which an impetuous fqually wind was then blowing, were furrounded by clouds, the infide of which was much agitated, and their top was lengthened like rifing fmoke, towards the place in which I obferved. The inhabitants of thofe mountains call thofe clouds *la gonfia* (the glafs-blower) and they look upon them as fure figns that a certain wind, of a rainy, or fnowy, and fqually nature, which they call *la tormenta* (the ftorm) will take place: this wind, from thofe mountains blows eaftwards. In Turin I never happened to obferve an impetuous wind blow from the above mountains, but I alfo defcried them to be furrounded by clouds

clouds like thofe I mention, lengthened like fmoke, into right lines more or lefs extended ; and reciprocally, I have always found fuch clouds to be accompanied by the kind of wind I mention.

1009. Thefe obfervations I made on the mountain of St. Michel, hindered me from expreffing myfelf in as pofitive a manner, in the letters I wrote to Signor Beccari, on the *nature*, as on the *conftancy* of the atmofpheric electricity during ferene weather ; becaufe I was not fufficiently affured, that fuch an inverfion of the nature of this electricity took place, only in the circumftance of the above rainy and ftormy clouds.

1010. It has only been fifteen years after the above mentioned obfervation, that I have happened to meet with another inftance of fuch inverfion.—On the morning of the 19th of January 1770, having climbed up to the top of Superga, in order the more eafily to obferve from thence, whether the *Aurora borealis* of the night before had returned, I happened to meet there Sig. Prior Ceca; who, with his ecclefiaftical ftudies, unites peculiar talents for fciences of other nature ; and he, with great kindnefs offered to me to watch the motions of an apparatus which I had fettled there. On the 24th of February, the apparatus was repaired ; and I frequently afterwards took a trip there, in order to obferve likewife, and fee the refult of the obfervations of the kind Prior. On the 13th of Auguft 1771, to come to the point, being on the fpot I mention, I happened to meet with another inftance of *defective* atmofpheric electricity, during ferene weather : this circumftance we wrote, in the account we kept of our common obfervations, to have taken place, " during the time a very ftrong wind blew from the mountains, which towards North-weft, hide the mountains of Lanzo, behind which we faw clouds rife, fhaped like exhalations of fmoke," and in fhort, fimilar to thofe juft now defcribed.

1011. On a former occafion, Sig. Priore Ceca being alone, had obferved another inftance of a fimilar inverfion of the ftate of atmofpheric electricity ; and it had taken place, (to ufe the very words of his journal) " at a time when a temporary cloud, brought " by an eafterly wind, broke, and was converted, as it appeared,

"into a *wind*, which paffing over the wire, electrified it by a deficiency:"—by the word *wind*, which is a pretty common expreffion in this country, the Prior meant a cloud, the top of which ftretched into feveral right lines, under the fhape of exhalating fmoke.

1012. No other inftance of defective electricity, in clear weather, befides the above, I have fucceeded to perceive, during three years I have fince continued to obferve on the mountain of Superga. Neither have I happened, in my obfervations in the Valentin, or in thofe which I have continued to make here in Garzegna, for feveral months every year, or in thofe I have occafionally made in Cigliero, Andrà, Alba, and other places, to meet with any inftance of the like electricity, during clear weather. Only, on the 18th of April of the prefent year (1775), fince I am again come back here to Garzegna, I have met with another inftance of the fame kind: here follows the manner in which I have fet it down in my Journal. "About 10 o'clock 40′ in the morning, the wind blows ftronger than ever; the bandrole or weather-cock, keeps waving with great quicknefs between north and eaft; I draw off a ftrong fpark with my finger; in lefs than a minute I obtain another, and perceive that the balls are electrified by deficiency; they foon fall down, and then the wires again grow flowly and weakly electrified by excefs; then again arifes a flow and weak deficient electricity. At 11h. 5′ a great divergence, certainly from deficient electricity, takes place; a little while after it leffens, but its nature is not altered. At 11. 10′ the deficient electricity is very much increafed, nor does its intenfity feem to be regulated by the ftrength of the fqualls of wind that take place. At 11h. 13, zéro; at 11 h, 30′, a fmall excefs. . . . Barom. French inches 26, 5. 4. Hygrom. + 12. Therm. $12\frac{1}{2}$ (*Reaumur's Thermom. very likely*); wind north, and blows ftrong. In fact, the Apennine mountains are furrounded by clouds, the upper edges of which are divided into numbers of rectilinear fpouts, like rifing fmoke, the tops of which are bent towards us; the clouds under thefe fpouts look black, as in a ftorm; and before them, other clouds of an oblong fhape are fpread,

ſpread, which are flat underneath, and full of eminences on their upper part, &c ; at noon the wind continues to blow impetuouſly; ſmall exceſs ;" and ſo on.

1013. I have expatiated a little on the above few inſtances, in which, contrary to the general rule, the atmoſpheric electricity proved to be in a deficient ſtate during clear weather, becauſe I thought it very worth while to do ſo. Indeed every body knows how uſeful a thing it is in Natural Philoſophy, to derive obſervations from Nature itſelf, and, by the aſſiſtance of a number of ſuch, to be able ſufficiently to ground ſome univerſal propoſition. And how advantageous it is likewiſe, to lay down the true exceptions to the propoſition in queſtion, and to point out the peculiar circumſtances in which they have taken place, ſo that the reaſon of ſuch exceptions may thereby become ſufficiently clear and manifeſt.

1014. And in fact, the different circumſtances that accompanied the above inſtances of defective electricity during clear weather, ſufficiently indicated how far they were exceptions to the general rule, that an exceſſive electricity conſtantly obtains in ſuch weather, and ſhewed that ſuch electricity was rather brought over, by means of the wind, from ſome part of the ſky which was at that time either cloudy, or ſnowy, or rainy. In the inſtances I mention, the wind brought electrified vapours, in the ſame manner as Mr. Kinnerſly ſent electrical effluvia to his friend, by throwing his hat to him: only, the wind produced the above effect from an extremely great diſtance. In fact, during my laſt obſervation, the clouds I mentioned roſe above the tops of the Apennine mountains, leaving their foot uncovered ; and indeed their diſtance from the ſpot on which I ſtood was at leaſt 64,000 *toiſes*.

1015. The above inſtance, which indeed is very rare, of electricity being thus brought over from a very great diſtance, does not after all, materially differ from thoſe frequent inſtances in which, though the ſky juſt over our heads may be clear, dark thick clouds, which draw continually nearer to the place of the obſervation, ſend an electricity which is found to be alternately

exceffive and deficient. The edge of one of fuch clouds is, we may fuppofe, 20, or 30° diftant from the place of the obfervation, and that part of the fky which lies over the latter, is, as hath juft now been faid, free from clouds; yet, under fuch circumftances, and at fuch a diftance from the cloud, the exploring wire begins to be electrified (by excefs and deficiency alternately) efpecially when the edge of fuch cloud is divided, and ftretched towards the place of the obfervation, like exhalations of fmoke. But inftances like thefe latter, which frequently happen, I only mention in this place, in order that, being common and evident cafes, they may ferve to confirm the explanation I have given of the former, which occur but very feldom.

1016. Nor do I think however, that the fix times I have happened to meet with an inftance of defective electricity during clear weather, are to be confidered as too fmall a number to draw any conclufion from. If it be confidered that thofe inftances, or exceptions to the conftant electric ftate of the atmofphere in clear weather, are the only ones I have met with during a great number of years, and that every one of them has been accompanied by the fame circumftance of diftant dark clouds, refembling exhalations of fmoke, it will be thought that thofe fix exceptions, taken together, are a fufficient number for afcertaining both their *unity*, and the manner in which they were effected.

1017. Such inverfions of, or exceptions to, the common ftate of atmofpheric electricity in clear weather, grow ftill rarer, as the places are lefs raifed above ground. Either at the Valentin during two whole fummers, or at any time in Turin, I never happened to meet with fuch inftances. Here in Garzegna, during the *vacations* of fo many different years I have fpent in it, I have only this year had an opportunity of obferving the phenomenon in queftion; but then Garzegna is higher than Turin about one hundred *toifes*. In Superga, which is 248 *toifes* higher than Turin, I have twice obferved the fame fact. On the mountain of St. Michel, which is 583 *toifes* high, I happened to make the obfervation three times in fifteen days. If the wind, before it reaches the place of the obfervation, ftrikes againft the earth, it will

will bring no peculiar kind of electricity from the cloud from which it comes: now the mountain of St. Michel, which is like a high ſharp cone, riſes on the very entrance of the valley of Suza. Superga is not ſo high, but then it is ſituated at a pretty conſiderable diſtance a-head of the mountains in the neighbourhood, and it thus was the firſt which was met by thoſe North-weſt, and Eaſt winds, from which I found the atmoſpheric electric deficiency proceed, which took place during clear weather.

9018. Propoſition V. *To find the inſtruments that may ſerve to obſerve and aſcertain the connexion between the atmoſpheric electricity in clear weather, and the preſent ſtate of the air; and aſſign the precautions that ought to be uſed.*

1019. I find that the electricity during clear weather is conſtantly connected with the ſtate of the air, as to moiſture and dryneſs. Therefore, in the inveſtigation I propoſe, obſervations of the barometer, thermometer, and anemometer, will be not only uſeful, but neceſſary: of all inſtruments however, that which is moſt eſſential is a perfect hygrometer, and ſuch a one ſtill continues to be wiſhed for every day.

1020. That contrived by Monſ. Deluc came to my knowledge only a few weeks ſince. This hygrometer is compoſed of a very thin tube of ivory, to which a glaſs tube, like that of a thermometer is annexed: the mercury is made to riſe in the latter tube, both by the heat, and by the dryneſs of the external air; which dryneſs contracts the tube or pipe of ivory. The mercury is likewiſe made to lower both by cold, and by moiſture, as this moiſture dilates the ivory pipe: the expedient therefore, imagined by the above Gentleman, has been to annex a correſponding thermometer to the hygrometer, and by its means, to aſcertain and deduct that part of the motion of the mercury, which is the reſult either of heat or cold, merely: and from the remainder he meaſures the degree of the atmoſpheric moiſture, or dryneſs.

1021. This hygrometer, among other advantages, ſeems indiſputably to poſſeſs thoſe of duration, and pretty exact menſuration: the author of it himſelf wiſhes it alſo poſſeſſed that of operating ſomewhat more quickly. This defect however may perhaps

perhaps be remedied by ſome ſubſidiary hygrometer; and perhaps it will alſo be poſſible, with ſome other very delicate inſtrument, to find out the proportion between the real quantity of moiſture, and the number of the degrees exhibited.

1022. However, being in abſolute want of ſuch a perfect hygrometer, I have hitherto made uſe of two, of my own contrivance: the one conſiſting of a ſtring, this I call the principal hygrometer; the other of bits or ſhreds of ſtraw, I call it the ſubſidiary hygrometer.

1023. The ſtring is made up of thirty-two flaxen threads twiſted together; the whole diameter of it is two third parts of a line; ſo that its thinneſs prevents its operating too ſlowly. I firſt try it by leaving it expoſed to the open air for ſeveral weeks, with two pounds weight ſuſpended to it: I then ſettle it on the outſide of the window to which the deferent wire is brought, in the following manner. I faſten an end of it to a nail, and then make it go three times round a pulley which is placed twelve feet above the nail, and its other end is loaded with a weight. This ſtring makes the axis of the pulley, together with an index annexed to it, move in company with the pulley; and the index ſhews the degrees of moiſture, from $+ 20^{\circ}$ of moiſture, to $- 20^{\circ}$ of dryneſs, on a round piece of paper, paſted in the inſide of the room on a pane of glaſs in the window: every one of the above degrees is again divided into ten conſpicuous parts.

1024. With regard to duration, an hygrometer like the above, commonly ſerves me for a year, or more; and I take care to make proper allowances, or corrections, when for inſtance I perceive, that, during very rainy weather, which continues with the ſame degree of heat, the hygrometer does not conſtantly indicate the ſame degree of moiſture, &c.

1025. With regard to ſlowneſs of operation, I obviate it, as I ſaid, with ſhreds of ſtraw. With a ſharp penknife I ſplit the top of an old and well ripened ſtalk of rye into four parts; I take one of theſe, I ſtrongly twiſt it, taking care not to break its fibres, and I inſert it, thus twiſted, through a ſquare piece of wood, out of which it gets about two inches on each ſide: on each

ſurface

ſurface of the piece of wood degrees are marked, and over theſe very fine ſhreds of untwiſted ſtraw move, which are inſerted through the body of the former twiſted ſhred. This kind of hygrometer moves on firſt breathing upon it, and goes over ſeveral degrees, before the index of the other hygrometer with a ſtring begins to move; it have found it particularly uſeful to diſcover the firſt moiſture of the nightly dew; I for that purpoſe place it out of the window at night, in an horizontal ſituation, and the upper part of it indicates the degree of the moiſture from above, and the under part indicates the moiſture which any how takes place underneath.

1026. Yet, notwithſtanding the care that may be uſed in that reſpect, hygrometers never can by themſelves indicate the abſolute quantity of moiſture in the air, nor its complete relation to the ſtate of the atmoſpheric electricity in clear weather.

1027. In the firſt place, every body knows how the air incloſed in a bottle of glaſs, drains the dew that has been introduced into it the night before; the whole atmoſphere operates in the ſame manner; yet hygrometers may very well, owing to the heat of the day, indicate an increaſe of dryneſs, while the air is really taking in continually new quantities of moiſture.

1028. In order therefore to conjecture with ſufficient exactneſs the abſolute quantity of moiſture in the air, a thermometer will be uſeful, but much more ſo, an hygrometer which is ſheltered from variations of heat and cold. However, by having a thermometer placed by your hygrometer, you may pretty exactly gueſs what quantity of moiſture the external air requires, and can keep within itſelf in a ſtate of exact diſſolution, in conſequence of its then degree of heat.

1029. I ſay pretty exactly, becauſe there is another cauſe, beſides heat, which makes air take moiſture in, and keep it in a ſtate of exact diſſolution, that is, its denſity, owing to the weight of the atmoſphere that preſſes upon it; and this additional moiſture it depoſits in a viſible manner, when quickly dilated to a certain degree. Therefore, the variations of the barometer may likewiſe aſſiſt us in gueſſing with greater exactneſs that quantity of moiſture, which

which the air actually requires and may keep to itſelf, in conſequence of the above denſity.

1030. Beſides the above kind of denſity of the air near the ſurface of the earth, there is another, which ariſes from a diminution of its heat. The ſame thermometer therefore, which by riſing, indicates that the air begins to *require* a new quantity of moiſture, may alſo by lowering, that is, by ſhewing that a condenſation takes place in it, indicate the very ſame thing.

1031. To theſe conſiderations on the moiſture of the air, another may be added, drawn from its inſulating nature; which is, that it retains and ſtops the electric fire leſs exactly, in proportion as it is heated beyond a certain degree.

1032. In eſtimating this power of air, of *retaining* the electric fire, account muſt alſo be made of its different degrees of denſity. Thus, as the upper part of the atmoſphere is, in fair weather, more completely free from moiſture than the lower, it will in conſequence thereof, more completely inſulate than the latter; but then, owing to its greater rarity, it will alſo allow the electric fire to ruſh through intervals proportionably greater.

1033. I mention all theſe different circumſtances, not that I think that obſervation can ever enable us to eſtimate them all exactly, and that any geometrical calculation of the motions of the electricity during clear weather, may be grounded upon them; but then it is not to be doubted but that paying a great attention to all the above different things, though without calculating them exactly, will prove of great ſervice in inveſtigating both the laws and the cauſe of the electricity in queſtion.

1034. To the above extenſive and nice conſiderations, great aſſiduity in obſerving muſt be added. Thus, to ſpeak of myſelf on this occaſion, whenever I have ſufficient leiſure to repair hither, to Garzegna, where beſides other conveniencies, I meet with the advantage of ſolitude, I ſpend my whole time in obſerving. I live night and day in the high and open room I have choſen; there the *deferent* wire brings me inceſſant information of the ſtate of the external electricity, and ſubjects it to a very ſenſible

elec-

electrometer. From thence furveying all parts of the horizon around me, at every inftant I have an opportunity of comparing the varying ftate of this electricity, with that of the weather or fky.

1035. By paying due regard to all the above mentioned confiderations, and to feveral others I fhall have occafion fucceffively to mention, as well as by obferving in the above mentioned manner, I flatter myfelf I have brought the complicated queftions on the motions of atmofpheric electricity during clear weather, to very fimple terms, which I now proceed to lay before the reader.

1036. Propofition VI. *The moifture in the air is the conftant conductor of the atmofpheric electricity during clear weather; and the quantity of fuch electricity is proportioned to that quantity of the above moifture which furrounds the exploring wire; except fuch moifture alfo leffens the exactnefs of the infulation both of this wire and of the atmofphere.*

1037. I do not mean in this propofition to point out the principle itfelf which produces the electricity in queftion, but only to afcertain that medium in which it is inherent, and to the quantity of which it is generally proportioned.

1038. I think it will not be amifs to fay fomething concerning the manner after which I have inveftigated the above truths: a few particulars of that kind will the more naturally lead the reader to the knowledge of the fubject. And in the firft place, I have endeavoured to procure, as nearly as I could, fome kind of meafure of the real quantity of electric fire, which in calm weather, manifefted itfelf around the exploring wire.

1039. The following is a fketch of the electrometer I have employed here in Garzegna. I have before mentioned that I had fixed a piece of cryftal in a wooden pane, placed in the window: this piece of cryftal, which is pretty thick, has been bored through in the middle; and through the hole made in it a pretty long male fcrew is inferted, terminated outward in a ring, to which the hook of the deferent wire is faftened. To the other end of

the male fcrew, which lies within the room, another fcrew is faftened, which terminates in a brafs rod : this brafs rod is of courfe placed in a perpendicular fituation to the furface of the above piece of cryftal ; and upon it, by means of a proper fupport, the electrometer is raifed. This electrometer, as I formerly faid, confifts of two fmall balls of pith of elder, one line in diameter ; a fmall femicircular fheet of brafs, placed perpendicular to the above brafs rod, keeps the threads feparated, that they may not entangle together; thefe threads are fixteen lines long, very fine and even, and I keep them wet with falted water.

1040. In order to meafure the divergence of the balls, a femicircular piece of paper is pafted at fome diftance behind them, on the piece of cryftal ; this piece of paper is properly graduated, and parted into two quadrants, by an interval equal and correfponding to the thicknefs of the above fheet of brafs.

1041. This kind of electrometer, which is extremely fenfible, together with that of Mr. Elfmey, which is put in motion only by a ftronger electricity, may enable an obferver to meafure the different degrees of electricity, with great exactnefs; efpecially, if the fmall pendulum of the electrometer of Mr. Elfmey be placed between two quadrants ; for while it is placed over one only, part of the electricity of the latter is employed in repelling it, and yet it is from that part alone of the motion of the ball, which is parallel to the quadrant, that the whole electricity is commonly determined.

1042. In making the above obfervations on the electricity of the atmofphere during clear weather, I foon perceived that it was equally effential to make account of the frequency of this electricity, that is, of the velocity with which it arofe again after being annihilated, as of its intenfity itfelf. In fact, the quantity of the electric fire which accumulates in the exploring wire, continually varies, according to circumftances ; and this quantity is not lefs conftantly proportioned to the divergence produced by the electricity in queftion, than to the frequency of it. This frequency I have commonly computed from the number

of

of feconds which elapfed before the balls again manifefted any fign.

1043. In the firft place, I have conftantly obferved that, whenever the electricity obtained, while the air was damp, whether fuch dampnefs arofe from the heat of the air, which enabled it to take in a greater quantity of moifture, or its denfity, or whether it was accumulated only in the vicinity of the exploring wire, this electricity was more frequent, than when the air was dry to any great degree.

1044. In fact, while fuch a dry ftate of the atmofphere as above obtains, more than a minute is fometimes requifite to have the electrometer again manifeft fome fign of electricity; whereas in the damp ftate of the air I mention, (efpecially if fuch dampnefs proceeds from heat, for then it leffens much lefs the exactnefs of the infulation) a fecond of time has fcarce elapfed when very rapid ofcillations of the balls again take place between my finger, and the fheet of brafs which ftands between them.

1045. Such conftant obfervation rendered it evident to me, that in the abovementioned damp ftate of the weather, a much greater quantity of electricity was exerted around the exploring wire, than in dry weather. But that fame great quantity of electricity was but very inconfiderable, when compared to that quantity, which, muft needs be diffipated from both the atmofphere and the wire, in confequence of their infulation being leffened by the dampnefs of the air we fuppofe.

1046. This conclufion which I drew by analogy from other electric experiments, was moreover immediately demonftrated to me by the different accidents I obferved in the atmofpheric electricity itfelf during clear weather. I conftantly found that, whenever a dampnefs arofe in the air from cold, efpecially when the vapours gathered and condenfated near the furface of the earth, the frequency of the electricity indeed increafed, but its intenfity was leffened: now, this decreafe of the intenfity of the electricity, combined with the increafe of its frequency, certainly indicates a very great deperdition of the fame.

1047. Whence we are to conclude, that, however true it may be, that the quantity of the electric fire which is exerted in the atmofphere during clear weather, is proportioned to the quantity of moifture contained in it, yet, as this very moifture is the conductor of fuch fire, it thence follows, that whenever a fomewhat confiderable and lafting condenfation of it takes place, every perceivable fign of electricity muft ceafe in the exploring wire.

1048. Propofition VII. *The electricity that takes place, when the weather clears up,* I. *is always of the exceffive kind.* II. *When the weather, in clearing up, moreover wants a great degree of drynefs, and acquires it at a great rate, the electricity rifes to a great degree of intenfity, accompanied by a proportional frequency, which latter however grows lefs as the drynefs of the weather increafes farther.* III. *It fometimes happens that the electricity caufed by the clearing of the weather, continues in its ftate of intenfity for a long while, or alfo that, after being interrupted, it begins afrefh; thefe accidents feem to be owing to fuch electricity being brought over by the wind from great diftances.*

1049. As to the firft part of the above propofition, it is confirmed by conftant experience. I am fo ufed to find the atmofpheric electricity in an exceffive ftate, when the weather is clearing up, that as foon as I perceive the thick lower clouds over my head begin to break, and the rare even clouds above them grow dilated, the rain having alfo ceafed every where elfe, and at the fame time find the balls of my electrometer diverge, I write down *excefs, tendency to clear weather*; nor has it ever happened that the fubfequent trials I made in that refpect, fhewed me that I had too haftily judged of the nature of the atmofpheric electricity under fuch circumftances.

1050. The fecond part of the above propofition, concerning the peculiar intenfity and frequency of the atmofpheric electricity, while the weather is clearing up, is alfo conformable to a number of obfervations I have made in that refpect: here follows the

manner

manner in which, in my Letters, pag. 126 I defcribed one of thefe obfervations, which I had made with a kite.—" While I was amufing myfelf with obferving how, in proportion as the weather cleared, the brifknefs of the electricity encreafed, I had the thought of running a pin through the pack-thread which held the kite, when I foon faw a very confpicuous brufh of fire fpring from its point towards my finger, the light of which continually encreafed; fo that, though I at laft prefented another pin to another part of the packthread, and a ftar appeared upon it, yet the former brufh was not entirely fuppreffed." This experiment lafted from one o'clock in the afternoon, till an hour after fun-fet, and fupplied me with a very favourable opportunity of obferving the joint progrefs both of the weather in clearing up, and of the atmofpheric electricity.

1051. The fubfequent obfervations I have made with exploring wires, have fince pointed out to me that circumftance which, when the weather is clearing up, particularly contributes to make the atmofpheric electricity, which of itfelf is in an exceffive ftate, to be moreover particularly intenfe; which circumftance is the fame as that expreffed in the prefent propofition, which is, when the weather, from a damp ftate, quickly paffes to a ftate of confiderable drynefs.

1052. To the above obfervation, the anfwer is very conformable, which Prior Ceca fent to a queftion I had propofed to him, concerning the ftate of electricity, when the weather clears up. " If when the rain has ceafed (the Prior faid to me) a ftrong exceffive electricity obtains, it is a fign that the weather will continue fair for feveral days; if the electricity is but fmall, it is a fign that fuch weather will not laft fo much as that whole day, and that it will foon be cloudy again, or even will again rain." And in fact, it is very natural that a connection takes place between the ftate of amofpheric electricity, and the production of lafting fair weather, fince the latter is effected by a complete diffolution of the atmofpheric moifture; efpecially when fuch diffolution is quickly effected.

1053. And the obfervation I made juft the day before yefterday, is quite conformable to the above prognoftic. From the 30th of April till then, the weather had been rainy. "This morning, May 8, the weather begins to clear up; the hygrometer, (which very early was at + 19 $\frac{1}{2}$) is now, at 7 o'clock 30″m. at + 17; detached clouds are paffing over the obfervatory. . . . . . Small excefs . . . . at eight o'clock, 40m. the return of fair weather is decided; the clouds are gradually diffipating, though no wind blows, &c. In confequence thereof, I find that the electricity is fo increafed that the balls of the electrometer diverge 20°; in its higheft ftate it reaches to 25°; but it frequently falls quickly, nor does it rife again, but after a minute of time. . . . The fky is without clouds to the diftance of about 45° degrees from the zenith. I think that the above quick falls of the balls arife from the moifture in the air, which during the clearing of the weather, is depofiting on the glafs-fticks by which the exploring wire is infulated, which moifture produces a kind of imperfect communication on the furface of thofe fticks, which only allows the above electricity to diffipate itfelf through it, when it has rifen to the above degree of intenfity. A great calm ftill continues to prevail, fo far as I can judge from the bandrole of the weather-cock. At 9 o'clock 50m. the electricity rifes again to 25°, then begins to fail. . . then rifes again. . . . Thefe alterations continue till 11. 15′; afterwards the intenfity of the electricity leffens, as well as its frequency; the hygrometer ftands at − 6. . . . At noon it ftands at − 10. . . In the afternoon the divergence is of 12°; the hygrom. ftands at — 12, 2′; about fun-fet the divergence is of 8°."

1054. The above alternative falls of the electricity ferve to fhow the caufe of that particular kind of electricity which obtains when the weather is clearing up, which is the quicknefs with which the moifture in the air is then diffolving. To this the reviving of fuch electricity, which rofe to 20°, or even to 25°, only after a minute's time, is very conformable: when an electricity obtains in dry weather, it is a very common thing that a longer time than that elapfes, after it has been deftroyed, before it again rifes

rifes only to 6°. - However, I will add with refpect to the above prognoftic, that the weather continued fair yefterday, and to-day.

1055. Laftly, the third part of this propofition has for its object, a kind of electricity analogous to the former, which, after the weather has completely cleared up, continues to obtain, or fometimes will rife afrefh: fuch accidents I have pretty conftantly obferved here in Garzegna, when a cold wind blew, caufed by the fnow that had fallen on the neighbouring mountains. Thus, on the 4th of October 1769, the electricity of *clearing weather*, which here had ended about noon, began afrefh about three o'clock, and I wrote, as I find in my Journal, that, "probably the electricity of clearing weather, on the mountains, which continued clouded pretty late in the day, did only reach us in the afternoon; and that the above cold wind was blowing from thefe mountains."

1056. In the laft propofition I have confidered the ftate of electricity during the clearing of the weather, which is the one of the limits between cloudy and ferene weather. I propofe now to confider its ftate, while clouds are gathering, which is the other limit between ferene and cloudy weather.

1057. Propofition VIII. *If, while the fky grows clouded over the place of the obfervation, only a high cloud is formed, without any fecondary clouds under it, and fuch cloud is not an extenfion of a cloud that drops rain elfwhere, in fuch cafe I fay, either no electricity takes place, or it is an electricity of an exceffive kind. If the clouds which are gathering, are fhaped like locks of wool, and continue in a ftate of motion from and to each other, or if the general cloud which is forming lies very high, and is ftretched downwards like defcending fmoke, then a frequent exceffive electricity commonly takes place, which is more or lefs ftrong, in proportion to the quicknefs with which this cloud is forming, and it forefhows the greater or lefs quantity and velocity of the rain or fnow which is to follow.* II. *When a rare, even, and extenfive cloud is forming, which darkens much the ufual colour of the fky, and turns it into a grey colour, an excefs of peculiar both intenfity and frequency takes place; but in proportion as the gathering of fuch cloud flackens, this excefs leffens, or even fails.*

On

On the contrary, if the rare extenſive cloud we ſuppoſe, continues to be gradually forming of ſmaller clouds, like locks of wool, which are continually joining to and parting from each other, the exceſs commonly continues. III. *Low and thick fogs, (eſpecially when, in their riſing, they find the air above them pretty free from moiſture) carry up to the exploring wire, when they reach it, an electricity which becomes manifeſted by frequent little ſparks, and produces a divergence of* 20°, 25°, *or even* 30°. *If the fog grows ſluggiſh, and continues around the exploring wire, all electricity ſoon fails: if it continues to riſe, and another cloud ſucceeds, it brings to the wire a freſh electricity, though leſs than the former. Sky-rockets ſent through ſuch thick, low and continued fogs have often afforded me ſigns of electricity. However, I never happened, under any one of the circumſtances above deſcribed, to meet with an inſtance of defective electricity; except perhaps once, when I ſent a ſky-rocket (to which, like to thoſe above, a long ſtring was fixed) through a low thick fog; though I had afterwards every reaſon to think that I had miſtaken* a falſe little ſtar, *for a true one.*

1058. With reſpect to the firſt part of the above propoſition, viz. the conſtant prevalence of an exceſſive electricity, in the circumſtances above deſcribed, I have aſcertained it but lately. The ſeveral manners in which clouds are formed, are complicated with numbers of different accidents, which I have been obliged cloſely to obſerve for many years; and it has only been by uſing much patience, that I have been able to diſcern eſſential and characteriſtic differences between them, and thus to derive ſome real information from my numerous obſervations.

1060. In regard to the ſecond part of the above propoſition, viz. the formation of thoſe thin, even, extenſive clouds, which for the time being, make the ſky appear of a grey colour, I have made abundance of obſervations, with the apparatus which I ſettled on the Valentin, in the ſpring of the year 1757. On the 11th. of May the very long rope was fixed, which extended over the river Po, from the Villa *of the Miſſion*, to the Valentin; and from this rope, an iron wire was continued to the portico of the botanic garden: here follow the words of my Journal, as they were written by Sig. Canonica, who aſſiſted me in my obſervation.

"On

" On the 12th of May, at 2 in the afternoon, frequent ſmall ſparks take place, while the ſky is overſpread with whitiſh vapours." Again, " on the 23d of the ſame month, a perfect calm; the ſky is overſpread with whitiſh vapours; the electric ſigns prove either ſtrong, or weak, according as the ſky is more or leſs copiouſly covered with ſuch vapours."

1061. In my firſt obſervations on Superga, I took notice, in the ſame manner, of the particular kind of electricity which is deſcribed in the ſecond part of the preſent propoſition. The apparatus had been fixed on the 24th of February, 1770. " On the 28th the ſky having been, at ſun-riſe, pretty clear and free from clouds, the atmoſphere begins to be loaded with a rare fog, which condenſates as it riſes; while it thus keeps riſing, and between 6 and 7 in the evening, the wire affords frequent ſigns; afterwards they ceaſe, the above ſaid rare cloud being already very high, and converted into numbers of ſmall whitiſh clouds, united to each other. On the firſt of March, at 9 in the morning, the air begins to be loaded with vapours, yet they are ſo rarefied as to form no cloud; the wire is electrified by exceſs; theſe vapours grow gradually thick enough to make the light of the ſun appear of a whitiſh colour, and both the briſkneſs and frequency of the ſigns increaſe; about 10. 45′. the vapours continue to riſe, and ſeveral whitiſh ſtreaks, formed by little clouds, begin to be formed, when the electricity ſo increaſes as to afford ſix or eight ſmall ſhocks, every minute. On the ſecond of the ſame month, in the morning, the vapours ſtill continuing to riſe, the electricity proves the ſame as yeſterday, &c. On the third, about 10, the wind falls; at $10\frac{1}{2}$ the wire reſumes a middling degree of electricity, which laſts in that ſtate the whole day; no well formed clouds, but only a great quantity of vapours appearing in the air," &c. The above obſervations have ſince acquired a continually new degree of preciſion, from thoſe I have made in the Valentin, on Superga, here in Garzegna, and in the Aſtronomical Obſervatory in Turin.

1062. I ſhall dwell ſomewhat longer on the third part of the preſent propoſition, viz. the electricity produced by a thick fog

which rifes ; as it has been the fubject of my firft experiments, which began in the year 1756, and which I have defcribed, in my firft Letters to Sig. Beccari. (See *Letters* to Sig. Beccari, num. 67.)

1063. " On the fecond of December, after a low thick fog had prevailed for feveral days, which had kept both fun and ftars conftantly hidden, and the weather continuing in that fame ftate, fo that a man could not be perceived at the diftance of thirty fteps, I raifed a fky-rocket, about $2\frac{1}{2}$ in the afternoon ; the firft made the flaxen thread move immediately, and with great force ; indeed, this thread, which was of itfelf much wrinkled, became ftretched into a completely right line, and fo did all its different filaments : I moreover could perceive a light on its lower extremity."

This light fuggefted the idea to me of ufing the electric lanthorn : I warmed its neck, and carried it to the place of the experiment, wrapped in warm cloths, which were to be taken off only after the fky-rocket was raifed. Having fent up the rocket, I eafily perceived a real *little ftar* within the lanthorn, which correfponded to the ftring which was faftened to the rocket: this little ftar, however, I only perceived after the rocket had rifen to a great height ; nor did I fee it but during an inftant."

" On the third of December, the weather continuing in the fame foggy ftate, except that the fog was fomewhat higher, I again fent up two rockets, and the fame accidents took place."

" On the fourth I repeated the experiment, in the Hall of experiments in the Univerfity, in order to exhibit it before the Students of Natural Philofophy, who come there at fettled days; when the experiment fucceeded very well ; only the electric figns were neither fo quick, nor fo ftrong as in the former ones: the fog was in great meafure vanifhed ; the fky indeed continued to be overfpread with clouds, but they lay high, and began to dilate : the above experiments was made between 9 and 11 in the morning; and at noon, after twelve days of foggy and cloudy weather, the fun made its appearance."

1064.

1064. Thefe experiments on fogs, which were the only fubject of the Letters I wrote to Sig. Beccari, are but few, when compared to thofe I continued to make, during the winter of that fame year. " In order (I faid in one of thefe Letters) to avoid the tedious narrative of all the numerous attempts and experiments I have, till now, continued to make, I fhall only indicate here the different methods I have ufed for making fuch experiments, leaving for another occafion the account of their precife refults, (*Let.* num. 73.) and in the firft place, I have during the courfe of the laft winter, frequently raifed rockets, and explored the electricity of the atmofphere, conformably to the method defcribed in my laft." Now, that phænomenon which moft excited my curiofity was the electricity of fogs.

1065. During that fame winter I had made experiments upon fogs, only with rockets. The firft time I have obferved their electricity with wires, was towards the end of that winter (*Let.* num. 75). " On the latter end of March, I climbed the high and fteep mountain of St. Michael, and there I ftretched and infulated feveral iron wires; the one in the direction of the Meridian of the Monaftery, to the ruins of the *Sepulchre*, which was 1600 feet long; another 120 feet long from the fame place, and in the fame direction, to a battlement of the Monaftery; another, 112 feet long, at the fame height, and eaftwards, to a ftore-houfe; another 160 feet long, from the fteeple to the ruins of a portico, weftwards; and laftly, I fixed a pole on the top of the Church, on which I had infulated a metallic point with a glafs ftick, and another iron wire 40 feet long, extended from this point to the neighbouring fteeple."

1066. It was then I fay, I had, for the firft time, an opportunity of obferving the electricity of fogs, with exploring wires: I had fettled them on fuch a high fpot, and made them of fuch great extent, in order conftantly to obtain, by their means, the fame electricity as I had till then fought to procure with kites, which can only be ufed when the wind blows fteadily. One day, to come to the point, as we were preparing to leave the mountain, and were going to take our wires down, a fog arofe

close to the east side of the mountain. Having perceived this, Sig. Borghesi, who is now a Physician at Saluzzo, and I, ran to the wire of the store-house, which the fog had scarcely begun to surround, when it afforded little sparks, continued and pretty brisk; but they soon ceased, having lasted rather less than a minute: they began afresh twice, but continued a less time, and were weaker than the former; which was owing to the fog then leaving the wire: and as that which succeeded it, grew sluggish and condensated itself around both the wire and the whole mountain, it put an end to all electricity.

1067. This observation created in me a greater desire of renewing it, than of giving, as yet, any public description of it. In the Valentin, either during that Summer, or the following, I had no opportunity of meeting with a like instance, especially because I could not go there early enough. As for this place, Garzegna, it is but a bad situation for observing fogs; they either never reach it, or only do so in cloudy or rainy weather.

1068. With respect to the other observations I have attempted to make, here in Turin in the Astronomical Observatory, when it was first established, and in Superga, I shall describe them on some other occasion: I mean here to speak of fogs, no otherwise than as one of the limits between serene and cloudy weather: however, since I am treating this subject, and two ingenious English Gentlemen, Mess. Ronayne and Henley* have also of late investigated it by different methods, I shall here subjoin a few questions I proposed about it, in my Letters to Sig. Beccari, num. 451. " Now, do not low, sluggish, and thick fogs also depend, at least in part, on the aerial electricity? It is enough for me to consider that neither the ground, nor the walls, nor cieling of my room, prevent my giving such electricity to the air in it, as lasts for a very long while, to understand how, notwithstanding the contact of the earth, an aerial electricity may exist, extremely apt to raise and actuate the above

* The above gentleman is the same, who, owing to a mistake in the Italian original, has been called *Elsmey*, in the 1041st paragraph, p. 434.

men-

mentioned fogs."—I then continued, num. 452, "Certainly, among the many effects which aerial electricity may produce, the following is abundantly confirmed by experience, viz. that all the vapours, or moist effluvia whatever, which are any how brought to rise in the atmosphere, or which swim, or descend in it, are affected by the aerial electricity, in their absolute as well as relative motions. Thus for instance, several admirable accidents of the dew, or of the hoar-frost, the tendency of their drops, or icicles, to certain particular bodies,—this tendency obtaining from all sides, though more particularly directed to the angles, edges, and points of such bodies, all these are accidents which suppose a perpetual electricity in the atmosphere, &c.

1069. Proposition IX. *When, during clear weather, a cloud happens to pass over the wire, which is low, tardy in its progress, and single, that is, considerably distant from any other, the electricity by excess commonly weakens much, but does not turn into a defective one; and when the cloud is gone, it returns to its former state.* II. *When numbers of whitish clouds, like locks of wool, keep over the wire, continually uniting with, and parting from, each other, and thus forming together a body of pretty considerable extent, the electricity by excess commonly increases.* III. *In all the above circumstances the electricity by excess never turns into a defective one.*

1070. In the two preceding propositions I have treated of the two limits between clear and cloudy weather, viz. its clearing up after rainy weather, and its growing cloudy after fair weather: in this, I consider the two different manners in which this latter change is effected. The whole of my successive observations authorises me to lay down an essential distinction in that respect, and to indicate what clouds increase, what lessen the electricity; a thing about which I expressed myself but incompletely in my Letters to Sig. Beccari, (pag. 167.)

1070. The clouds which weaken the electricity are those which proceed slowly: for I have in different instances happened to see, during clear weather, portions of clouds which the wind had detached from the body of those around the mountains, and which it transported rapidly and pretty exactly united into one

body,

body, over the exploring wire, remarkably increafe its electricity: the fame I find, has alfo been obferved by Sig. Prior Ceca.

1071. Such clouds, moreover are low; for all thofe which I have found more remarkably to leffen the electricity, appeared to be much fo, from the great diftinctivenefs of their parts, the darknefs of their lower furface, and the limits to which this darknefs was confined, relatively to the then fituation of the fun.

1072. Laftly, I farther diftinguifh thofe clouds by faying that they form only one body, and are entirely fevered from other clouds. With refpect to all thofe characteriftics of the clouds I mention, they are very eafily diftinguifhed. Number of fuch clouds, for inftance, are to be feen in fummer-days, efpecially when fhowers of rain take place at no great diftances; they are not much raifed above the horizon, and are difpofed feemingly in pretty regular order, at a fufficient diftance from one another; their upper parts are covered with prominences, and their under furfaces are kept flat by the wind which conveys them : I commonly exprefs them by the fhort appellation of *rafts*, which fits them the better as they bring fupplies for the above fhowers. Such are the clouds, I fay, which paffing over the exploring wires, weaken their electricity, after which it returns to its former ftate, as I have frequently obferved in the Valentin, and here in Garzegna; nor have I failed to perceive the fame effects in Superga, whenever I have happened to be there, in proper weather.

1073. I have not lefs frequently, nor with lefs pleafure, ftood contemplating thofe clouds of another kind, which are of a whitifh colour, and are in a continual perturbated motion : for which reafon, as well as on account of their peculiar appearance, our countrymen call them the *kids*. When one confiders them attentively, he foon perceives that they are not kept in that ftate of inteftine motion by the action of the wind, but by fome peculiar internal principle which caufes them to join together by fome one part of them, and to part in another. Now, fuch clouds, contrary to the former, commonly increafe the electricity of ferene weather. However, as their firft rife, as well as final diffipation, are never fo confpicuous accidents, as thofe of the paffage

paſſage of the above clouds, or rafts, ſo neither can the beginning, as well as end of the change they produce in the preſent ſtate of the atmoſpheric electricity, be ſo well aſcertained as that brought on by the former.

1074. Such are, moſt excellent Sir, the phenomena exhibited by the atmoſpheric electricity during ſerene weather, the perpetuity of its exiſtence, the conſtancy of its nature, its connexion with the dryneſs and moiſture of the atmoſphere, its prevalence even under the circumſtances of the weather beginning to grow cloudy, or clearing up, and the different oppoſite changes that are produced in its ſtate, by ſolitary clouds of different kinds. Though the above different obſervations and facts may at firſt ſight appear in great meaſure remote from the important queſtions your Excellence has propoſed to me, yet, I flatter myſelf that I may be able, in time, to ſhew the cloſe connexion there is between them, and will thus be ſo happy as to have once more an opportunity of expreſſing my great reſpect and gratitude to your Excellence.

Garzegna di Mondovi,
May 16, 1775.

LET-

LETTER II.

On the Daily Period of the Atmoſpheric Electricity during Serene Weather.

To the illuſtrious Preſident of the Royal Society of London, Sir John Pringle, Bart.

1075. I do not know how better to expreſs my gratitude to ſeveral Members of this Royal Society, for the kindneſs they have at different times ſhewn to me, than by expreſſing theſe ſentiments to the Society itſelf, by your means, illuſtrious Sir, and making an offering of ſome ſpecimen of the reſearches I have made concerning the moſt active of elements. My inclination for the Science of Electricity, which Science properly belongs to this century, has never ſuffered any diminution; and I ſhould be particularly happy, if beſides the ſatisfaction of purſuing a favourite ſtudy, I have had the advantage of deſerving the approbation with which the lovers of electricity in general, and the founder of it, the immortal Dr. Franklin, as well as the moſt ingenious Dr. Prieſtley, who ſo ſuccefsfully promotes the knowledge of both electricity and the element we breathe, have honoured my former endeavours. Indeed, I hope that the new diſcoveries I now offer to the Public concerning the nature, the conſtancy, and other different phænomena of the atmoſpheric electricity in clear weather, will not be found by the above perſons, by any means unworthy of the time I have employed in my long and numerous reſearches. The mild electricity I mention is not in itſelf leſs to be regarded, than the ſtormy and thundering: and the ſlow, but continuous effects of the former, are of no leſs importance than the violent and loud operations of the latter. Every thing in Nature is great; nor is it any diminution of the merit of a diſcovery, that the objects diſcovered do not make upon our ſenſes any ſtrong impreſſions.

preffions. With refpect to the mild and conftant natural electricity, which is the fubject of this treatife, I moreover flatter myfelf, that an attentive inveftigation of its effects will be in the higheft degree ufeful to attain a knowledge of feveral important circumftances relative to the other kind of natural electricity, which are unknown to this day.

1076. In the firft place, in order to imitate the method of the ingenious perfons above named, I will fhortly relate the manner in which I have been led to make the obfervations which I propofe to offer to the reader, and in which I have afterwards promoted them. So early as the year 1753, in my firft book on Electricity p. 173, num. 544, and following, I offered conjectures, "that feveral phenomena which daily take place in our atmofphere, may be explained by means of an electricity of a weaker kind." But this reafoning was no more than a conjecture, which I drew from feveral accidents I obferved to take place in the dew, and from the conftant manner of Nature, which leaves no agent unemployed, and carries operations of every one to every poffible degree. I then obferved only with the common apparatus, which as it was moreover in a very indifferent fituation, only could indicate to me the higher degrees of intenfity of the atmofpheric electricity during ferene weather; befides, that my obfervations on that electricity which takes places during fhowers, and rain in general, kept me then conftantly employed.

1077. It was in the year 1756, that the frequent and continued ufe of kites, which other obfervers only ufed to make refearches on the electricity of clouds, procured me a confirmation of what I had till then only conjectured, that is to fay, that even during clear weather (except in the cafes of a great dampnefs of the air, or of an impetuous wind) a mild weak electricity perpetually took place. (Let. p. 166).

1078. Kites were moft ufeful inftruments to me, for fuch firft experiments on the ftate of the atmofphere. They rife to a great height, to a region where the difference of the atmofpheric

electricity uſes to be greater; they gather great quantities of this electricity, by means of the long pack-thread which holds them, and they retain it the better as they are capable of being exactly inſulated. With reſpect to inſulation, experience has ſuggeſted the following formule to me, $E = \frac{D, R, L.}{S.}$, that is to ſay, the exactneſs of the inſulation is proportioned, I. to the dryneſs of the air, directly; II. To that repulſion of dampneſs, which is proper to inſulating bodies, directly. III. To the length of ſuch bodies, directly; and IV. To their ſection S, inverſely, or to their perimeter, if they are ſolid compact bodies. Now, a ſtring made of the beſt ſilk, of a ſmall diameter, and of great length inſulates a kite extremely well; and it is an eaſy matter to keep it dry by warming it, or to change it, when it grows damp. Though I was at firſt ignorant of the contrivance of Sig. Romas, who interweaves the ſtring which holds his kite with thin metallic wires, the ſame thought occurred to me the more naturally, as I was then exploring the accidents of the weaker electricity that takes place in ſerene weather.

1079. But it ſoon happened with me, as is commonly the caſe, that one obſervation created a wiſh for others. Having aſcertained the electricity of windy clear weather, I thought of employing ſky rockets to explore its ſtate during a calm; and at laſt, in order to make conſtant and durable obſervations on its variations, I had recourſe to exploring wires, as I related in the above quoted letters to Sig. Beccari.

1080. Beſides the above expedients, I had moreover tried, though without ſucceſs, a pole with an electrometer; however, I find that Mr. Ronayne has employed it very ſucceſsfully for inveſtigating the electrical ſtate of fogs; which electricity I had at firſt explored with ſky-rockets, and I have afterwards aſcertained its nature by means of my extenſive exploring wires.

1081. Till the year 1754 I had frequently written to Sig. Beccari concerning aerial *artificial* electricity, and its laws; the contents of my letters I publiſhed in the year 1758. I thought

in

in the beginning, that the electricity which was propagated in the air of my room, adhered to the fubftance itfelf of that air; and I judged in the fame manner of that which takes place during ferene weather. As I had not then fufficiently confidered the weaknefs of fuch electricity, I thought it could be obferved, like the other kind of atmofpheric electricity, with an electrometer not infulated. To that end, I raifed a long pole on the top of a houfe, on the extremity of which a ftick was placed, with two thick linen threads hanging from it. However, I reaped no information from the experiment, partly becaufe the threads entangled together, partly becaufe I did not vifit them frequently enough, and did fo only in very clear weather, when they were leaft likely to indicate any thing.

1082. But, to fay the truth, the aerial and *artificial* electricity I raifed in the room, was in reality an *aereo-vaporous* electricity (890); that is to fay, it was diffufed through the moift effluvia which fwam in the air in it; and in like manner, the natural electricity of the atmofphere during clear weather, refides chiefly in the vapours diffufed in it; and as fuch electricity, when the weather is completely clear, is but weak, it cannot, like the electricity I raifed in the air in the room, be obferved with electrometers that are not infulated; for thefe two electricities are refpectively *proportioned to the deficiency of fire which the excefs in the vapours may, in both cafes, raife and maintain in the air in which the electrometers are immerfed.* Now the natural excefs of the atmofphere, when the weather is completely clear, and cold, requires more than an whole minute to be able to reproduce any flight electricity, by means of a lofty exploring wire. Electrometers, therefore, that are not infulated, cannot indicate the electricity of clear weather, or at leaft cannot indicate it with that conftancy with which long and lofty exploring wires, that are exactly infulated, are able to do, which gather in themfelves, unite and retain fuch electricity; fo that it at laft rifes to a fufficient degree of intenfity to be difcernible. And now, that to thefe obfervations, I have joined thofe made with a pole, like that

contrived by Mr. Ronayne, I find my reafoning to have been conformable to facts.

1083. But it is time to conclude this difcuffion of the methods I have employed; now I proceed to relate what new difcoveries I have been able to make, efpecially concerning the daily period of atmofpheric electricity during ferene weather.

1084. Propofition X. *In the morning, if the hygrometer continues to indicate a great degree of drynefs, which is equal to, or little lefs than, that of the preceding day, then, even before the rifing of the fun, an electricity takes place, which is manifefted by junctions, adhefions, or even a divergence of the balls, and is proportioned to the abfolute degree of the drynefs in the air, and the fmallnefs of its difference from that of the preceding day: if no fuch great drynefs obtains, no difcernible electricity takes place before, or even for a little while after, the rifing of the fun.*

1085. As the weather during the night is commonly damp, fo it is a pretty common cafe, that, before and after the rifing of the fun, no electricity obtains. The following are the days in which I have found that an electricity took place before fun-rife, during the fpace of three months, the time from which I have refumed my obfervations, that is, from the 11th of April, to the 11th of July.

April 11, 12, 13, 14, 18, 20, 21, 26, 27, 28, 30.
May 9, 17, 24.
June 1, 4, 24.
July 3.

1086. On the 14th of April, the long drought ftill obtained, which had prevailed during the preceding months, though it then began fomewhat to abate; therefore, on the firft four mornings when I renewed my obfervations, I found an electricity obtained before the rifing of the fun, and I think I would have found it equally conftant during the preceding months, had I obferved. In May and June, fome dampnefs in the air arofe at night, and the fame continues to be the cafe in July: this moifture is brought over by eafterly winds, efpecially the South-eaft wind; this latter

latter has ſeveral times cauſed my hygrometer to fall 10, or 12 degrees, and then rain has followed.

1087. From the whole of my numerous obſervations, I find, that it is rarer to meet with an electricity before ſun-riſe in ſummer, than in winter; eſpecially, if the dampneſs from hoar-froſt is prevented, as I have three times experienced during the month of January, 1769. I had raiſed a long pole on the roof of the Obſervatory, and on it a glaſs-ſtick, a foot and an half long was fixed, which was protected by a deep and narrow umbrella of tin, ſo that the hoar-froſt could not ſo ſpeedily coat it. The iron wire which proceeded from the above glaſs ſtick, was kept ſtretched by a ſilk ſtring, faſtened to a chimney, the remaining heat of which ſomewhat preſerved its inſulation. Laſtly, this wire was bent, and was continued ſo far as to the inſide of the obſervatory, through a window ſituated on the South ſide of it. Having three times during that month, tried, before ſun-riſe, the electricity of this wire, the extremity of which I held with a ſilk ſtring, which was kept dry by ſome fire, I found a pretty conſiderable one obtained, which one time out of the three, afforded pretty frequent ſparks, though for two or three minutes only, that is, as I think, when the hoar-froſt was beginning.

1088. With reſpect to the obſervations I have made theſe three months, the reſult has been, I. That I never found an electricity obtain in the morning, unleſs the hygrometer indicated a degree of dryneſs beyond — 12°. II. Commonly, when I have met with an electricity before ſun-riſe, the hygrometer was not fallen of an whole degree under that exhibited on the preceding evening. III. Once, that is on the 14th of April, I found the hygrometer fallen three degrees, but then it was with great difficulty I could diſcern a few electric adheſions; to this add, that, when they were taking place, a gentle North-weſt wind began to diſſipate the dampneſs of the air, as I aſcertained by means of my hygrometer with ſhreds of ſtraw.

1089. However, in order properly to examine the connection between the dampneſs, or the dryneſs of the atmoſphere, and the

the failure, or the prevalence of the morning electricity, one muſt take care that no ſtrange dampneſs ariſes in the place of the obſervation. Thus, for fear my own perſpiration during the night ſhould diſturb the electricity, at night I take care to tranſport the extremity of the deferent wire to the window of the next room to that in which I ſleep, and to which I may, when there is occaſion for it, ſpeedily go and obſerve.

1090. Propoſition XI. *In the morning, according as the ſun riſes higher, the electricity, whether it began before ſun-riſe, or only after, gradually increaſes.* II. *This gradual increaſe of the morning electricity begins ſooner, according as the hygrometer continues, after ſun-riſe, to indicate a higher degree of dryneſs, and as ſuch dryneſs more ſpeedily increaſes.* III. *Theſe increaſed both intenſity and frequency of the electricity laſt, in ſerene days, in which no impetuous wind takes place, ſo long as the ſun does not draw near the place of its ſetting, and the hygrometer keeps near the higheſt degree which it had reached.* IV. *When the ſun is near its ſetting, and in proportion as the hygrometer begins to retreat, the intenſity of the daily electricity leſſens, and its frequency increaſes.*

1091. The truth of the above propoſition will appear evident to every attentive obſerver, and I think I may diſpenſe with producing any inſtances to ſupport it. I will only remark that whoever will wiſh to be properly informed of the gradual advances, and diminutions, of the dryneſs of the atmoſphere, muſt make uſe of pretty quick hygrometers; to that end I again recommend thoſe with twiſted ſhreds of ſtraw: now I proceed to point out the different cauſes which produce variations in the daily period of atmoſpheric electricity.

1092. Prop. XII. *Though the hygrometer may indicate equal degrees of dryneſs, on the middle of the day, in different days, yet the frequency of the daily electricity may be greater in ſome of thoſe days than in others, and is in great meaſure proportioned to the increaſe of heat which takes place in them: the electricity moreover ariſes, on ſuch days, later in the morning, and fails ſooner in the evening.*

1093.

1093. On the 11, 12, and 13th of April, the hygrometer ſtood very near —20°; on the 28th of June, and 2d of July, at 2. 30′ in the afternoon, it again roſe to —20°. Now, though in the middle of each of thoſe different days, the hygrometer indicated equal degrees of dryneſs, the operation of the electricity in ſome of them, proved extremely different from what it did in the others.

1094. In the three former days, before the riſing of the Sun, an electricity took place which was manifeſted by motions, or adheſions, of the balls to my finger; and two hours after ſun-riſe, they diverged 4°, 5°, 6°. II. Nor did the electricity riſe higher during the remainder of thoſe days. III. At ſun-ſet the degree of the electricity was ſcarcely lowered. IV. The ſlowneſs of it was moreover ſuch, that, in the evening, after the ſheet of braſs between the balls had been touched, 30″ at leaſt, elapſed before a freſh adheſion took place; and in the middle of the day, an whole minute was requiſite.

1095. In the two latter days, I. Bare adheſions ſcarcely began at 8 o'clock. II. At night, they ended before 7. III. But during each of theſe two periods, the electricity was renewed in leſs than a ſecond. IV. However, in the middle of the day, the electricity roſe to 8°, but ſtill its frequency continued to be pretty conſiderable, and after 2″ it began again to be manifeſted.

1096. And indeed, though in the middle of every one of theſe different days, the hygrometer indicated very nearly the ſame degree, of dampneſs, and the ſurface of bodies was equally deprived of moiſture, yet the air was impregnated with very different quantities of it. A proof of this is, in the firſt place, ſupplied by the hygrometer itſelf. On the 11, 12, and 13th of April, the hygrometer did not fall, even during the night, under — 19°. On the contrary, its variations during the two above mentioned warm days, were as follows,

28th of June, 5. 30′ in the morning — 9. 5.
2. 30′ in the afternoon. — 19. 3.

at

at 5. 30′ on the following morning, — 10.

4th of July, 5. 30′ in the morning — 12. 4.

2. 30.′ in the afternoon — 19. $\frac{1}{4}$.

at 5. 30′ on the following morning, — 8.

1097. In thefe two laft days the hygrometer therefore indicated a much greater dampnefs during the night time, than it did in the former. II. The air, of courfe, in the middle of thofe days, kept a greater quantity of moifture in a ftate of exact diffolution. III. And the heat of fuch days was undoubtedly the caufe of this : in fact, the thermometer of Reamur rofe to 24°; whereas in the former, it did not rife above 10°.

1098. Hence, though the hygrometer exhibited the fame degrees of dampnefs, in the middle of the former days, as in that of the latter, yet the ftate of the atmofphere was very different during the nights. Nay, it was fo likewife in day time; and to fuch differences the operations of the electricity proved very conformable.

1099. And to begin with that part of the above phenomena which is moft obvious, it is plain that the abundant moifture which, during the latter hot days, was kept in a ftate of exact diffolution, began, towards the evening, and as the heat gradually leffened, to defcend and condenfate itfelf near the furface of the earth; by which it weakened the infulation of the inferior part of the atmofphere, as well as that of the exploring wire: hence, the whole electric fire that remained in the atmofphere, every where diffufed itfelf into the earth. As for the night time, no electricity was *manifefted in it*, becaufe the general deperdition of it ftill continued, and even increafed, as the remarkable alterations of the hygrometer witneffed.

1100. However evident the above explanation may in itfelf be, I fhall add a few more facts to fupport it. On the 23d of June, the hygrometer, from — 5 rofe to — 17, 3; which denoted great drynefs; for the 22d had been exceedingly wet, and I have conftantly obferved that one day is not fufficient to carry the hygrometer quite up to the actual degree of drynefs, if the

the dampneſs was very great before. Conformably to the above great riſe of the hygrometer, the electricity which, at 7 in the morning, was at zero, at 8, 30. had riſen to 6°. and during the whole day, kept at 80° or very near. On the evening at 6, the electricity had fallen to 5° ; between 8 and 9, it fell to 2° ; at 10 o'clock it roſe again to 5°, but it ſoon lowered again to 3°, to 2°, and there it remained till I ceaſed to obſerve, that is, till 11, 25′.

1101. On the morning I began again to obſerve at 3, 15′, when I found that the electricity ſubſiſted, and was manifeſted by briſk motions of the balls ; and at ſun-riſe it increaſed.

1102. Now, this inſtance of the electricity continuing during the whole night in clear weather, is no very common inſtance in ſummer. It even happened then, that in that very night of the year, which is ſo celebrated with the vulgar, on account of St. *John's dew*, the atmoſphere depoſited ſcarcely any moiſture. The thermometer, which, at noon, was near 23, at 11, 25′, continued to ſtand ſo high as 19, 5 ; and at 3, 15′ in the morning, it ſtill ſtood at 19.

1103. The dryneſs of the above night was moreover rendered very conſpicuous by the hygrometer : during the whole night it did not lower one degree under — 17, 3, which was the higheſt point it had riſen to on the day before ; and on the morning, before ſun-riſe, I found it at 16, 5.

1104. Again, on the 30th of June, the electricity continued pretty high during the night ; at 10 o'clock, it ſtood at 2°, and correſpondently to this, the hygrometer, from the higheſt degree of dryneſs, — 17, 4, which it had expreſſed on the day before, was ſcarcely lowered 0, 2 : on the morning, at 4, 30′, the hygrometer was lowered to — 15, and I accordingly found the electricity to have ceaſed ; nor was it before 6, 10′ that it began again to be manifeſted.

1105. Indeed it is no very rare thing, in ſummer, to meet with inſtances of the electricity failing about the time the ſun ſets, owing to a certain ſufficient degree of dampneſs which then ariſes ; and it is afterwards renewed, as the air, a few hours afterwards, again acquires a certain degree of dryneſs : but this kind

 of

of electricity I here mention, belongs to that which I call electricity *of dew*, of which I propofe to treat more explicitly, when I fhall fpeak of the laft part of the daily electrical period. However, the two above rare inftances I have juft now defcribed, in which the hygrometer continued, during the night, to fhow a degree of drynefs very nearly equal to that in the day, feem to me pretty plainly to indicate the caufe why the daily electricity begins (as it indeed commonly does) later in the morning, and rifes fooner at night.

1106. But to return to the greater frequency of the electricity in warm days, its difference from that which takes place on other days, is fo great, that, on the very firft year I began to obferve, I perceived it: and of this fact, not only particular experiments, but the whole feries of any obfervations that may be made, will furnifh affured proofs; efpecially if fuch obfervations are made in calm weather, for, as we will foon mention, fome particular winds increafe the frequency of the electricity, as well as heat. Sometimes in fine fummer days, I have fufpended a pendulum for feconds, near the electrometer, which I touched with the fame hand that kept the pendulum raifed; my other hand was in the meanwhile placed near the balls of the electrometer, ready to make them manifeft their electricity as foon as it fhould begin to take place; now, as foon as my other hand was removed, and the pendulum began to defcend, the balls performed a vibration between my finger and the electrometer. On the contrary, in fine winter days, I have time to crofs the room once or twice, before I find a new adhefion takes place.

1107. Thefe great differences in the frequency of atmofpheric electricity, I look upon as undoubted proofs of what has been advanced in Prop. VI. of the preceding letter; which is, that to that quantity of moifture which is brought by the air clofe to the exploring wire, the electricity of clear weather is proportioned, except in as much as this moifture leffens the exactnefs of the infulation. In fact, in warm days the air takes in and diffolves a much greater quantity of moifture, as we find from thofe great variations that immediately take place in hygrometers, when to hot

hot days fucceed very cool evenings; and this heat (as we may perceive with our naked eyes, and as I have ftill better afcertained with the microfcope of the Count of Pertengo) keeps the above moifture in a ftate of continual ebullition. On the contrary, when the weather is both very clear and cold, to the flownefs of the electricity correfponds a fcarcity of atmofpheric moifture.

1108. Nor is it only from fuch confiderable differences of the electricity, as correfpond to confiderable differences of heat, that I am confirmed in the above opinion; but I am alfo from the farther circumftance of fmaller differences in this electricity, alfo correfponding to fmaller differences in the heat. In fact, on the morning of fummer days, the electricity when it firft begins, proves fomewhat lefs frequent than a few hours after; which is no doubt owing to the lefs quantity of moifture which the air keeps at that time in a ftate of exact diffolution.

1109. With fuch hypothefis, the manner is alfo very confentaneous in which the electricity both leffens and then fails, towards the end of the day, and as the night advances. In fact, even in cold and very dry weather, when the electricity is very weak, yet, as its intenfity gradually leffens towards the clofe of the day, its frequency increafes (or rather, its flownefs diminifhes) proportionally to the above decreafe of its intenfity; and this frequency keeps increafing, according as the dampnefs of the night more quickly augments, that is to fay, according as the moifture which, during the day, was any how diffeminated in the air, more fpeedily gathers near both the exploring wire, and the furface of the earth; the confequence of which is, that the electricity of the wire is more quickly annihilated, and alfo, that the fame moifture then more fpeedily brings a fmall new electricity to it, which is proportioned to its imperfect infulation.

1110. If to all the above obfervations, we add the confideration of the peculiar electricity *of dew*, with which I propofe to conclude, and that of winds, which, in proportion to their dampnefs, as I fhall prefently relate, increafe the frequency of

 the

the electricity of clear weather, we ſhall certainly find it as evident a truth as any in Natural Philoſophy, that the moiſture in the air is the conductor of this electricity.

1111. The electricity, however, either in ſummer and winter, does not always exactly keep pace with the quantity of moiſture in the air, either in the night or the day. When I make my experiments during ſummer, in a ſhady place, they entirely fail, or only anſwer very imperfectly; though in order to make them ſucceed, I only need expoſe the whole apparatus to the ſun. That is to ſay, the moiſture in that air which is expoſed to the ſun, is more divided, is brought to greater rarity, and greater intervals take place between its parts than in that in a ſhady place; it therefore leſs weakens the inſulation. Now, the exploring wire is in the day-time expoſed to the ſun, and in ſummer to a ſun which is the warmer.

1112. Prop. XIII. *The friction of winds againſt the ſurface of the earth, is not the cauſe of the atmoſpheric electricity.* II. *Impetuous winds uſe to leſſen the intenſity of the electricity of clear weather.* III. *If they be damp, they leſſen its intenſity in proportion to the diminution they cauſe in the exactneſs of the inſulation, both of the wire, and of the atmoſphere.*

1113. Ever ſince the atmoſpheric electricity was diſcovered, I have endeavoured, after numbers of different manners, both in very dry, and in damp weather, to raiſe ſome electricity in deferent bodies, by means of currents of air artificially produced. Several times I have tried rapidly to turn four bands of gilt paſteboard, fixed like wings, to an axis inſulated by means of ſticks of glaſs. At other times I have for a while kept driving air againſt ſheets of metal, with a pair of bellows, and ſometimes, againſt bands of linen cloth, either very dry or wet, which were ſtretched within a frame, and inſulated with ſilk ſtrings. I have alſo tried to ſet fire to ſquibs which turned with great velocity around a ſtick, or peg, exactly inſulated: now, neither in this peg, nor in the above mentioned axis, which conſiſted of an iron rod, nor in the above ſheets of metal, nor in the bands of linen cloth,

cloth, could I ever perceive the leaſt degree of electricity, both when the current of air took place, or after.

1114. Again, in the Letter addreſſed to his Excellence, Count of Scarnaſigi, which is joined to thoſe directed to Sig. Beccari, I demonſtrated by concluſions drawn from accurate experiments, that the light which I had diſcovered to take place when a column of air precipitated itſelf into a vacuum, indeed was an electric light, but then that it never took place on the ſurface of deferent bodies, and only on that of thin inſulating ones.

1115. Experiments of this kind are certainly very apt to raiſe doubts about the opinion which certain *cloſet* philoſophers ſo gratuitouſly propagate, which is that the whole atmoſpheric electricity is produced by the friction of the air againſt the ſurface of the earth.

1116. Such opinion is beſides abundantly confuted by the obſervation which forms the ſecond part of the preſent propoſition, which is, that winds commonly weaken the atmoſpheric electricity during clear weather: with reſpect to this general *aſſertion*, I ſhall here enter into ſome particulars.

1117. In three different caſes, I have found the electricity of clear weather, intirely annihilated by the wind. In the firſt place, it has happened, when owing to the impetuoſity of the wind, the kite with which I explored the ſtate of the electricity, hath in conſequence of its tail not being ſufficiently loaded, taken a plunge, and been brought near the earth. I deſcribe one of thoſe caſes in my letters to Sig. Beccari (p. 106) in which the wind, beſides being impetuous, moreover appeared to be extremely dry; but the ſame has alſo happened when the wind was evidently damp. I ſhall obſerve, by the by, that if the electricity in any degree aroſe from the friction of winds againſt the ground, its intenſity would be found greateſt near the ſurface of the earth.

1118. The above rare experiments made with kites, are moreover exactly conformable to the conſtant and univerſal obſervation, that exploring wires become impregnated with a greater

greater electricity, according as they are raifed higher above the furface of the earth.

1119. But laftly, Sig. Count di Pertengo has obtained an inftrument for me, from the very kind Sig. Marquis di Prié, with which I almoft every day experience the inefficacy of winds to raife electricity. The inftrument I mean is a fine umbrella made of filk cloth, with a handle of charred wood, which is coated over with fealing-wax: the Merchants in Paris who fell thefe kinds of umbrellas, call them *Paratonnerres* *. Now, though I have often prefented this umbrella obliquely to a ftrong wind, I never happened to find the leaft electricity or motion in filk hairs, wetted with falt water, which I had annexed both to its middle parts, and to its edge.

1120. In the fecond place, I have likewife found impetuous winds to have deftroyed the electricity of exploring wires, though the latter were placed very high above ground, and of kites, though I then made them rife to a great height: fuch impetuous winds arofe at the time fhowery clouds were in fight, which they almoft inftantaneoufly diffipated. I find nine fuch inftances, in the journal of my obfervations in the Valentin.

1121. Laftly, I have found impetuous winds intirely to deftroy the electricity, when the weather was very clear, and quite dry. With refpect to this, however, I fhall inform the reader, that I have not judged of the drynefs of fuch winds according to the method of the vulgar, who miftake wind for drynefs. Indeed, wind carries off the moifture of fuch bodies as are more abundantly impregnated with it than itfelf is: hence winds generally dry wet clothes to fome degree, and conftantly promote the evaporation from the furface of waters: but winds of really great drynefs, which draw off all moifture from the furface of bodies, are, at leaft in this country, extremely rare. Hygrometers of twifted ftraw often have indicated to me that certain winds

* An umbrella, or fhelter againft thunder; the above word is imitated from the French words *parafol*, or *parapluye*, umbrellas for the fun, or for rain.

con-

contained moiſture, which, from every other circumſtance, I judged to be completely dry.

1122. In my experiment of the 29th of November 1756, which I have related in my Letters to Sig. Beccari (p. 129), that wind, which, almoſt in every reſpect, appeared to me completely dry, yet, very likely was not really ſo, unleſs perhaps in a few particular waves of it, and indeed I could now and then diſcern ſymptoms of moiſture from it, though, as I ſay, they were not conſtant, and proved but very weak.

1123. The winds that prevailed on the high mountain of St. Michael, where I made experiments in March, the following year, were of ſtill greater dryneſs. There, when the frequent impetuous Weſt wind blew, the electricity grew exceedingly weak, or even was completely annihilated; and the three only times I ſaw it, under ſuch circumſtances, to be ſomewhat intenſe, was when its nature was altered (1008). And then, in the calm intervals that at times took place, the uſual mild electricity of clear weather again inſenſibly aroſe. In the mornings, a very ſtrong and continued Eaſt wind commonly roſe, which preſently deſtroyed the electricity of the wires. Four times, while this wind blew, I ſucceeded to make a kite riſe to a pretty great height, by faſtening a ſtone to the extremity of its tail, but it did not ſend me the leaſt electricity. Such winds, I only ſaid, were drier than that before mentioned; for as to being perfectly dry, I do not know whether any wind ever is ſo.

1124. Nor is it neceſſary to climb up to the tops of high mountains, to meet with ſuch winds as are ſufficiently dry, either to annihilate any perceivable electricity, or create in it ſuch a peculiar decreaſe both of its intenſity and frequency, as may demonſtrate the truth of the above obſervation.

1125. Even in Turin, in the ſpring of the year 1757, on two occaſions in which a ſtrong Weſt wind blew, I could not, owing to its dryneſs, perceive any ſign of electricity in the flaxen threads annexed to the ſtring by which a kite I had raiſed was held, except a few rare and weak motions.

1126

1126. As to this place, Garzegna, where I ufe to experiment in Autumn chiefly, I never meet with any inftance of wind that I might call perfectly dry, except perhaps once; the kite then happened to be raifed pretty high for the fhort fpace of one minute, and indeed I could not then perceive any electricity. But in this prefent fummer, three times a dry wind has annihilated, as I found, all electricity during the intervals of its greater violence, and that happened too, in the afternoon, when the electricity ufes both to continue at fuch a degree of intenfity as to keep up a divergence in the electrometer, and to be very frequent, owing to the heat in the air.

1127. Here follows the manner in which the laft of the three inftances I mention were fet down in my Journal.

" July.	hour,	barom.	therm.	hygrom.	bandrole.
11.	4, 15.	26, 9. 5.	25.	– 20. 7.	E. N. E.

Wind, impetuous and dry, and of courfe warm. . . . The electricity has for thefe few minutes proved quite unperceivable in the electrometer; at 4, 36′, adhefions begin again to take place, and I correfpondently begin to find that the wind grows lefs dry, for it grows fomewhat cool. . . The hygrometer of twifted ftraw, which is fituated in a place towards Eaft, in ten or twelve feconds, retreats one degree from the point of drynefs it indicated; and brifk motions of the balls already begin to take place. As to the hygrometer with a ftring, though I am always contriving corrections in it, when, the weather continuing the fame as to heat and clearnefs, I find it to go beyond—20, yet, it reaches today to — 20, 5.

1128. I have tranfcribed the above extract becaufe (befides the mention that is made in it of the imperfection of the hygrometer with a ftring) it ferves me as a natural tranfition to fpeak of moift winds. Even thofe which feem to be very dry, at laft often introduce fome moifture. Eafterly winds, during thefe three months, which, when fair weather is fettled, ufe here in Garzegna, to rife pretty ftrong about noon, at three or four in the afternoon, by wheeling to South-eaft, become damp to a very

very perceivable degree; and in the beginning, that is in April, or the firſt days of May, though the electricity ſtill continued to be of a pretty ſlow kind, they often remarkably increaſed its frequency, ſo that the balls of the electrometer, which, before this damper wind roſe, acquired a freſh electricity only after 20″, or 30″ of time, now recovered it after every ſecond.

1129. At other times theſe damp winds I mention have, at their firſt ſetting on, carried the electricity ſo far as 8°, or 10°, and at the ſame time they increaſed its frequency; but this has happened but ſeldom, and continued only for a ſhort time. In Superga, an evidently very damp wind has, ſeveral times, made the bells ring for a conſiderable time; which is certainly in great meaſure to be attributed to the very great length of the exploring wire I uſed there, and perhaps alſo to the conſiderable height of that mountain.

1130. Here in Garzegna, where eaſterly winds muſt, as they come on, rub againſt a ſeries of hills which are ſomewhat a-head of it, they, of courſe, diffuſe into them great part of the electricity which they may contain: hence theſe winds leſſen and at laſt annihilate the electricity, in proportion to the time they laſt, and to the moiſture they bring along with them.

1131. To the above reaſoning the obſervations I have conſtantly made in the Valentin, are very conformable: ſtrong and damp winds, in that low place, ſoon diſſipate the electricity.

1132. Thus have I laid down, moſt worthy Preſident, the theory of the alterations which heat, and eſpecially winds, create in the daily period of the atmoſpheric electricity. Here I might add, as in their proper place, a few conſiderations on the ſudden electricity *of evening dew*, which in certain ſeaſons, and on favourable days, grows complicated with the former, if I did not perceive that the many particulars I have entered into, in this letter, have already rendered it but too long: this length however, I hope you will excuſe, partly on account of the ſubject, partly on account of the deſire I entertained, while writing it, that it might convey a ſufficiently clear idea of the ſubject, both to you,

and to the ingenious Members of this Society, ſo that they might judge how far the above obſervations are well grounded, as well as rectify them if neceſſary; for though I do not think myſelf intirely deſtitute of any degree of pride, yet, truth is the object I ſeek after.

Garzegna di Mondovi,
July 14, 1775.

LETTER III.

On the Electricity produced by evening Dew.

To the Same.

1133. FROM the beginning I thought that bare reaſoning and analogy could demonſtrate the exiſtence of the electricity that is produced by dew. The firſt experiment that made me ſuſpect that obſervation might moreover render it ſenſible to us, was that which I made with a kite, here in Garzegna, on the 18th of October 1756; in which, a quarter of an hour after ſun-ſet, the ſtring which held the kite ſupplied ſuch a quantity of electricity that it could form both a *bruſh* and a *little ſtar*, on two pins, reſpectively, which were properly directed to produce the above effects. (Let. p. 126).

1134. The above opinion is moreover confirmed to me by what I find written among the obſervations made at the Valentin. "Laſt night, the Gardner aſſures us, he has drawn ten ſucceſſive ſmall ſparks."—I could not be on that ſpot during the nights.

1135. In March of the ſame year, on the mountain of St. Michael, I frequently continued up late at night, hunting after the electricity of evening dew, which I thought muſt obtain there, but I never could diſcover any ſigns of it: I imputed it, and pretty juſtly as I now find, to the ſtrong continued winds that prevailed.

1136. In the Autumn of the ſame year, while Sig. Canonica continued the obſervations in the Valentin with his uſual exactneſs, I went to Genoa, in order to make experiments concerning ſea light, and take information on that ſubject, from ſeamen. It was only in the following year (1758), that I ſettled an exploring wire, 1000 feet long, here in Garzegna, with which I ſeveral times diſcovered, eſpecially towards the latter end of October, that an electricity *of dew* took place, which was of pretty conſiderable intenſity. Ever ſince that year I have obſerved and ſet down, that ſuch electricity took place in clear and dry weather, during which no ſtrong wind prevailed.

1137. In ſubſequent years I continued to obſerve the ſame electricity of dew, in thoſe evenings in which the above united circumſtances happened to take place. At that ſame time I reflected, and afterwards ſaw confirmed by obſervation, that that kind of electricity which prevails in the evening about the ſame time as the electricity of dew commonly does, and which, though it attains a leſs degree of intenſity, yet manifeſts a pretty conſiderable frequency, is of the ſame kind with this latter.

1138. At Superga, during thoſe evenings I could ſpend there, I only happened to obſerve this ſecond kind of electricity. Sig. Prior Ceca could not ſo intirely diſpoſe of his time, during thoſe hours, as conſtantly to watch the motions of the electricity of dew, and he only paid attention to it when it became manifeſted to a great degree, and haſtened from his room to obſerve it, only when he was called up by the ſound of the electric bells; however, as I will relate in its proper place, he has ſeveral times happened to obſerve the real electricity *of evening dew,* though he conſidered it in a different light.

1139. Laſtly, this preſent year, on the beginning of April, I have again repaired to Garzegna, in order to make new obſervations, (for it is by conſtant obſerving alone, that I learn how to obſerve properly) and it ſeems to me that, with reſpect to the electricity of evening dew, the few following propoſitions may be laid down as founded upon facts.

1140. Prop. XIV. *In cold ſeaſons, if the ſky is clear, no ſtrong wind prevails, and a pretty great degree of dryneſs continues to obtain, an electricity of conſiderable intenſity ariſes after ſun-ſet as ſoon as the dew begins; the frequency of ſuch electricity is moreover greater than that of the* daily *electricity, and it vaniſhes with great ſlowneſs.*

1141. Prop. XV. *In temperate or warm ſeaſon, if the ſame circumſtances as above take place, an electricity, entirely ſimilar to the former, ariſes as ſoon as the ſun has ſet; only, its intenſity is not ſo conſtant, it begins with more quickneſs, it riſes to a greater frequency, and ends ſooner.*

1142. Prop. XVI. *If, under the above circumſtances, reſpectively, the general dryneſs of the air happens to be leſs, then the electricity that ariſes in the evening, when the dew begins, is leſs, in proportion to the diminutions of the exactneſs of the inſulation of both the exploring wire and the atmoſphere that then take place; but then, correſpondently to the greater quantity of dew, the frequency of the electricity is greater.*

1143. Prop. XVII. *The electricity of dew depends, it ſeems, on the quantity of the dew, as the electricity of rain depends on the quantity of rain; and the peculiar manner after which this dew takes place, influences the electricity, in the ſame way as does the peculiar manner in which rain likewiſe takes place.*

1144. Prop. VIII. *As rain, ſhowers, Auroræ boreales, zodiacal light, have a tendency to begin afreſh for ſeveral ſucceſſive days with the ſame characteriſtic accidents, ſo the electricity of dew ſeems to have, as it were, an inclination to appear for ſeveral evenings ſucceſſively, with the ſame characters.*

1145. Prop. XIX. *Let the air in a well cloſed room be electrified, that is to ſay, the moiſture and other vapours diffuſed in it* (887).

II.

II. *Let a bottle filled with water colder than the air in the room, and insulated on a tube of glass, be raised pretty high in this room.* III. *Let care be had to preserve the insulation of the glass with warm clothes.* IV. *The electric signs that will arise in two threads suspended to such bottle, will exactly represent the electricity of dew.* V. *And they will exhibit the different manners after which this electricity takes place, according as the electrified vapours in the room will be more or less rare, according as the difference between the heat of the air in the room, and that of the water in the bottle, will be less or greater, and according as the insulation of the bottle will be more or less exact.*

1146. The above propositions I have thus set down jointly, that the one may serve to add a new degree of clearness to the other. And, in order to throw still greater light on the subject, I have subjoined a compendious table of twelve different observations, the only ones I have had an opportunity of making concerning the electricity of evening dew, during the 120 days I have resided here since the 11th of April.

I. Months.	II. Days.	III. Electricity of dew.	IV. Its beginning, hour, min.	V. Hygrometer with string.	VI. Its variations.	VII. Variations of that with twisted straw.
April	11	18°.	7, 25.	19. 8	0 0	0 4
	12	16°.	7, 30.	19. 6	0 0	0 3
	13	15°.	8, 35.	19. 2	0 0	0 3
	17	12°.	7, 30.	18. 4	0 0	0 5
	20	18°.	9, 0.	18. 3	0 1	0 7
	26	15°.	7, 30.	17. 7	0 1	0 7
	27	12°.	8, 35.	14. 0	0 0	0 5
	28	12°.	8, 20.	17. 0	0 0	0 9
	9	22°.	8, 40.	17. 3	0 1	1 8
	19	13°.	8, 40.	18. 4	0 0	2 0
	22	23°.	7, 50.	17. 0	0 2	2 6
	23	20°.	8, 10.	15. 0	0 2	2 0

1147. The small quantity of observations in the above table, instead of proving the existence of the electricity of dew, seems rather

rather to give riſe to objections againſt it. In fact, ſince the fall of dew is ſo common an accident, how has it happened, that during 120 days, the electricity of dew has taken place only twelve times?

1148. But in the firſt place it is to be obſerved, that an electricity of this kind, ſomewhat intenſe, is only to be obſerved when the general dryneſs of the air is very great, as is evident from the column IV of the table above, in which the ſtate of the hygrometer with a ſtring, at the time of the riſe of this electricity is ſet down,—and alſo when ſuch great dryneſs is of a permanent kind, as is likewiſe manifeſt from the ſteadineſs of the above hygrometer during the time the electricity continued to be particularly ſtrong (its variation having been either very ſmall, or zero) and from the ſtate of the hygrometer with twiſted ſtraw during that ſame time, the variation of which was but ſmall, and has likewiſe been ſet down in the VIth column. Now, ſuch evenings as are altogether clear, greatly and permanently dry, and free from any ſtrong wind, are upon the whole but very rare.

1149. The evenings, during the whole warm ſeaſon, ought therefore to be excluded from the account, as wanting that great, and moreover permanent, dryneſs above mentioned. In fact, the copious moiſture which the air diſſolves during the day, leſſens the inſulation ſo much, that the frequency of the electricity greatly increaſes, though not its intenſity; and this moiſture, when firſt condenſated by the coolneſs of the evening, ſcarcely allows any ſufficient inſulation to ſubſiſt for the electricity of dew to become manifeſted. The twelve times therefore, in which I have, ſince the beginning of this ſeaſon, had an opportunity of obſerving an electricity peculiarly intenſe, ought not to be reckoned out of 120, but only out of perhaps leſs than fifty days. If inſtead of coming here to Garzegna, in order to obſerve, on the 11th of April, that is, on the laſt days of the long drought that had prevailed, I had come on the beginning of March, I do not doubt but I might be able now to reckon a much greater number of obſervations, in the ſame manner as I have formerly been, in more proper ſeaſons than this, though I ſpent a much leſs number of days in obſerving.

1150

6

1150. Befides, it ought to be obferved that I have fet down only thofe days in the above table, in which the electricity has been fufficiently ftrong to carry the divergence of the balls to twelve degrees, or above, as is marked in the 2d column: I have purpofely confined myfelf to fuch days, becaufe the electricity I mention differs greatly from the common daily one, and is thereby very apt to engage the attention of an obferver. Whenever an electricity arifes in the evening, after the ceafing of the daily one, though it may prove much lefs than the above, and even be merely manifefted by adhefions of the balls to my finger, yet it inconteftably arifes from the dew, fince it is conftantly proportioned to the quantity of this latter; and fuch electricity arifes every day in which a fufficient drynefs in the air, that is a fufficient infulation, obtains, and when no ftrong wind blows.

1151. This mention which I make of drynefs in the air, and of infulation, leads me to give the folution of a difficulty concerning the electricity of dew, that feem to arife from the infpection of the columns V. and VI. That is to fay, if fo ftrong an electicity as above obtained, confequently fo confiderable a quantity of dew fell on the evenings of the twelve above mentioned days, how came it to pafs that fo fmall variations took place in the two hygrometers as are fet down in thofe two columns? The hygrometer with a ftring remained feveral days without any motion at all; and the hygrometer with twifted ftraw, frequently moved only a few parts of a degree.

1152. In the firft place, it is evident from the variations in the hygrometer with twifted ftraw having been greater than in that with a ftring, that the fteadinefs of the latter was then owing to its fluggifhnefs merely; and this I have, befides found confirmed by a number of other facts. Neither are we to think that the hygrometer of twifted ftraw itfelf, was totally free from the above defect; befides that, as it was placed in a much lower fituation than the exploring wire, it was affected later by the moifture of the dew.

1151. In order to judge of the quantity of moifture that is neceffary for making the twifted ftraw advance half a degree, I

try

try to breathe upon it ſeveral times conſecutively; when I obſerve, I. That at the fiſt breathing it makes ſome little motion. II. But it moves ſo far as the whole half degree, only when I have breathed four or five times upon it. III. If, after the firſt or ſecond breathing, I ceaſe to breathe, the hygrometer conſtantly ſhows a tendency to move back. IV. When I breathe upon it, for the ſixth or ſeventh time, then it, as it were, leaps at once to the diſtance of an whole degree.

1154. From theſe different accidents I conclude, that, even in the hygrometer of twiſted ſtraw, a kind of ſluggiſhneſs obtains, —that ſome time is requiſite to enable the moiſture to penetrate into the inmoſt pores of it, and dilate them,—that the quantity of moiſture neceſſary to make this hygrometer move half a degree is not quite inconſiderable,—and laſtly, that the dampneſs muſt be ſomewhat continued, elſe the hygrometer will move back.

1255. But when I moreover conſider that the electricity from the dew grows more manifeſt, every other circumſtance being equal, in proportion as the length of the exploring wire is greater, and that the length of that which I uſe, ſurpaſſes eight hundred times the length of the above twiſted ſtraw, I underſtand how the loweſt degree of dampneſs which the latter is able to indicate, is ſufficient to create and maintain a pretty conſiderable electricity in the exploring wire.

1156. The above fact I more particularly diſcover when I obſerve the kind of proportion that obtains between the different manners in which electricities of ſtrong kinds take place, and the then variations of the hygrometer with ſtraw. For, as I have ſaid in Prop. XVII. and as I obſerved in April laſt, when the ſeaſon was above the temperate degree, the electricity of dew (except that it is always of the exceſſive kind) ſeems, in its different changes, to follow proportions much like thoſe which take place between the electricity of calm mild rain, and that of rainy ſtormy weather.

1157. In fact, on the three firſt evenings in which I have obſerved in April, I found that as the twiſted ſtraw ſcarcely could move three or four tenth parts of a degree, ſo the electricity from the dew

dew rofe with great quicknefs, it leffened very flowly, and when annihilated, it did not rife again to the fame degree, or nearly, but after fifteen minutes of time: at 11 o'clock 30′, a divergence of 12 or 10 degrees continued to obtain; and on the morning, before the rifing of the fun, pretty quick adhefions ftill took place, and a great drynefs ftill continued to prevail, fo that the hygrometer with a ftring had not moved half a degree.

1158. The very reverfe happened in my laft obfervations in May. I will produce as an inftance that I made on the 22d. I found the electricity to have rifen to 15, 16, 18, and 23 degrees; and between 7 o'clock 50′, and 8 o'clock, that is, in ten minutes of time, it fell to 15: fince that time it kept decreafing and increafing between 8 and 15 degrees; and by farther fucceffive variations it was at laft, about 11 o'clock, brought to only a fmall divergence. Now, all the above variations of the electricity correfponded to variations in that quantity of moifture in the air, which took place at that time, as was indicated by the twifted ftraw, which now and then ftarted back towards drynefs. With regard to the frequency of the electricity during that evening, I obferved, at 10 o'clock, that two feconds after touching the electrofcope, the balls began to diverge afrefh.

1159. The *confiftent* variety of the above accidents fully demonftrates the conftant connection that obtains between the dew and the atmofpheric electricity; for the manner after which dew takes place, varies as regularly as the manner of rain does, according to feafons. Nor is it neceffary that the dew fhould be very copious in order to produce a ftrong electricity; on the contrary, a very great quantity of it would at laft deftroy the infulation of both the wire and the atmofphere. The electricity itfelf of rain, the intenfity of which is fo fuperior to the latter, likewife requires that the exploring wires fhould be in fome meafure infulated; and in proportion as the rain becomes more extenfively diffufed, this electricity grows lefs fenfible; and it only recovers its former intenfity, when a change takes place in the nature, or rather the manner of the rain. Now, dew is conftantly diffufed in great plenty towards the ortive hemifphere,

 and

and its electricity may be compared to that of a very rare and ſubtle rain; and it therefore requires ſome degree of inſulation ſhould obtain in that part of the atmoſphere which is little raiſed above ground, in which alone we can obſerve it: dew muſt therefore be in ſmall quantity, that it may not diffuſe away its own electric fire.

1160. The *daily* electricity, to ſum the whole in few words, is therefore, like an electricity of a very rare fog, which riſes, grows dilated, and thus leſſens continually leſs and leſs the inſulation: the nocturnal electricity is like an electricity of rare and ſubtle rain, which deſcends, gathers together, and continually leſſens more and more the inſulation. Hence the daily electricity is of a continuous kind; the nocturnal frequently fails; and it only attain its greateſt degree of intenſity, when the increaſe of that moiſture which is the conductor of it, happens to take place without injuring the inſulation.

Garzegna di Mondovi,
April 2, 1776.

P. S. The time at which the ſtrong electricity of evening dew begins, varies very irregularly, as may be ſeen in Prop. III. Sometimes I have found it to begin before ſun-ſet; but then, ſeveral clouds, pretty high and thick, lay in the part of the Weſt, and the thermometer, as well as the hygrometer, anticipated their uſual variations. At other times the ſame has only begun after eleven o'clock at night; nor have I always been able to find out any ſatisfactory reaſon for it.—I likewiſe think that the *morning* dew under proper circumſtances, gives riſe to a peculiar kind of electricity. The three obſervations on hoar-froſt, which have been mentioned in a former place, may be conſidered as proofs of this; however, in order that this electricity may become manifeſt, care muſt be had that the preceding evening dew hath

hath not injured the inſulation of the apparatus. In the above mentioned obſervation on the 23d of June, I think I diſcerned an electricity of morning dew: the dew of the preceding evening had been exceedingly rare, as I found by means of the hygrometer of twiſted ſtraw; on the morning at 3 o'clock, the balls of the electrometer ſcarcely manifeſted any tendency to adhere to my finger, and yet at four, they briſkly vibrated between it, and the braſs plate that ſtands cloſe to them.

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	laſt.	B. C.	Tab. II. fig. 9.
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